

THE ACCOUNTING REVIEW

VOLUME 89, No. 2 MARCH 2014

STEVEN BALSAM and ERICA E. HARRIS

The Impact of CEO Compensation on Nonprofit Donations

FRANCOIS BROCHET, GREGORY S. MILLER, and SURAJ SRINIVASAN

Do Analysts Follow Managers Who Switch Companies? An Analysis of Relationships in the Capital Markets

JENNIFER L. BROWN and KATHARINE D. DRAKE

Network Ties Among Low-Tax Firms

SABRINA S. CHI, MORTON PINCUS, and SIEW HONG TEOH

Mispricing of Book-Tax Differences and the Trading Behavior of Short Sellers and Insiders

JONGWOON (WILLIE) CHOI

Can Offering a Signing Bonus Motivate Effort? Experimental Evidence of the Moderating Effects of Labor Market Competition

GUS DE FRANCO, FLORIN P. VASVARI, DUSHYANTKUMAR VYAS, and REGINA WITTENBERG-MOERMAN

Debt Analysts' Views of Debt-Equity Conflicts of Interest

JERE R. FRANCIS, MATTHEW L. PINNUCK, and OLENA WATANABE

Auditor Style and Financial Statement Comparability

MICHAEL D. KIMBROUGH and ISABEL YANYAN WANG

Are Seemingly Self-Serving Attributions in Earnings Press Releases Plausible? Empirical Evidence

YANIV KONCHITCHKI and PANOS N. PATATOUKAS

Taking the Pulse of the Real Economy Using Financial Statement Analysis: Implications for Macro Forecasting and Stock Valuation

ELLA MAE MATSUMURA, RACHNA PRAKASH, and SANDRA C. VERA-MUÑOZ

Firm-Value Effects of Carbon Emissions and Carbon Disclosures

KENNETH J. MERKLEY

Narrative Disclosure and Earnings Performance: Evidence from R&D Disclosures

NEMIT SHROFF, RODRIGO S. VERDI, and GWEN YU

Information Environment and the Investment Decisions of Multinational Corporations

PRIOR 10

**Marygrove College Library
8425 West McNichols Road
Detroit, MI 48221**

A JOURNAL OF THE
AMERICAN ACCOUNTING ASSOCIATION

AMERICAN ACCOUNTING ASSOCIATION

Board of Directors

President	Mary E. Barth, Stanford University
President-Elect	Christine A. Botosan, The University of Utah
Past President	Karen V. Pincus, University of Arkansas
Vice President–Finance	Dale L. Flesher, The University of Mississippi
Vice President–Finance-Elect	Stephen R. Moehrle, University of Missouri–St. Louis
Vice President–Research and Publications	Steven J. Kachelmeier, The University of Texas at Austin
Vice President–Education	Alan Reinstein, Wayne State University
Director–Focusing on Membership	David E. Stout, Youngstown State University
Director–Focusing on International	Recep Pekdemir, Istanbul University
Director–Focusing on Segments	Anne L. Christensen, Montana State University
Director–Focusing on Intellectual Property	Robert C. Lipe, University of Oklahoma
Director–Focusing on Academic/Practitioner Interaction	Shaun Budnik, Deloitte LLP

The Executive Director of the Association is Tracey E. Sutherland.

The American Accounting Association was founded in 1916 as the American Association of University Instructors in Accounting. Its present name was adopted in 1936. The Association is a voluntary organization of persons interested in accounting education, research, and practice.

Membership in the Association is available at the following annual rates.

Full membership:

Membership dues of \$210.00 include *Accounting Education News*, and the online selection of all three Association-wide journals: *The Accounting Review*, *Accounting Horizons*, and *Issues in Accounting Education*. (This does not include Section journals.) Members may also opt to receive hard copies of AAA journals for an additional cost:

<i>The Accounting Review</i>	with Print Option \$45.00	Total U.S. \$255.00
<i>Accounting Horizons</i>	with Print Option \$25.00	Total U.S. \$235.00
<i>Issues in Accounting Education</i>	with Print Option \$25.00	Total U.S. \$235.00
All three Association-wide journals	with Print Option \$95.00	Total U.S. \$305.00

Associate membership:

Full-time students are eligible for associate membership at \$40.00 which includes *Accounting Education News* and access to all three Association-wide journals: *The Accounting Review*, *Accounting Horizons*, and *Issues in Accounting Education*. (This does not include Section journals.) Members may also opt to receive hard copies of AAA journals for an additional cost. (Teaching Notes for *Issues in Accounting Education* are not available to students.)

<i>The Accounting Review</i>	with Print Option \$45.00	Total U.S. \$85.00
<i>Accounting Horizons</i>	with Print Option \$25.00	Total U.S. \$65.00
<i>Issues in Accounting Education</i>	with Print Option \$25.00	Total U.S. \$65.00
All three Association-wide journals	with Print Option \$95.00	Total U.S. \$135.00

In addition, full members may belong to one or more Sections: Accounting Information Systems, Behavior and Organizations; Accounting Programs Leadership Group; American Taxation Association; Auditing; Diversity; Financial Accounting and Reporting; Forensic and Investigative Accounting; Gender Issues and Worklife Balance; Government and Nonprofit; Accounting Information Systems, International Accounting; Management Accounting; Strategic and Emerging Technologies; Teaching, Learning and Curriculum; and Two-Year College. Each Section assesses its own dues, which are collected by the Association.

Editorial correspondence and revisions of manuscripts originally submitted before June 1, 2011 should be sent via email to the Senior Editor, John Harry Evans III, University of Pittsburgh, at tar@katz.pitt.edu • Telephone: (412) 624-8605 • Fax: (412) 648-1693. New manuscripts must be submitted through the Manuscript Submission and Peer Review System, located at <http://accr.allentrack.net>. The site contains detailed instructions regarding the preparation of files for submission.

The submission fee of \$200.00 for members of the AAA or \$400.00 for nonmembers may be paid online (VISA or MasterCard only) at <https://aaahq.org/AAAforms/journals/tarsubmit.cfm> and should be paid before the article is submitted.

Requests for information concerning membership, subscriptions, dues, advertisements, and other matters related to the Association (other than submission of manuscripts) should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399 • Telephone: (941) 921-7747 • Fax: (941) 923-4093 • Email: Info@aaahq.org. Notice of change of address should be mailed at least eight weeks in advance to ensure undelayed receipt of *The Accounting Review*.

The Accounting Review is indexed in the Social Science Citation Index, which is included in Web of Science.

The Accounting Review (ISSN 0001-4826 print and ISSN 1558-7967 online) is published six times a year in January, March, May, July, September, and November by the American Accounting Association, 5717 Bessie Drive, Sarasota, Florida 34233-2399. General subscriptions are available at the annual rate of \$430 for a print copy. The member subscription rate for a hard copy is \$45. Electronic access is included with membership dues. Periodicals Postage Paid at Sarasota, Florida and additional mailing offices. **POSTMASTER:** Send address changes to ***The Accounting Review***, 5717 Bessie Drive, Sarasota, Florida 34233-2399.

THE ACCOUNTING REVIEW

A JOURNAL OF THE
AMERICAN ACCOUNTING ASSOCIATION

Senior Editor
John Harry Evans III

Editors	
Michael L. Ettredge	Kenneth A. Merchant
David A. Guenther	Gregory S. Miller
Leslie D. Hodder	Donald V. Moser
Amy P. Hutton	Morton Pincus
Bin Ke	Vernon J. Richardson
Lisa Koonce	Phillip C. Stocken
Charles M. C. Lee	Beverly R. Walther

VOL. 89	MARCH 2014	No. 2
---------	------------	-------

STEVEN BALSAM and ERICA E. HARRIS	
The Impact of CEO Compensation on Nonprofit Donations	425
FRANCOIS BROCHET, GREGORY S. MILLER, and SURAJ SRINIVASAN	
Do Analysts Follow Managers Who Switch Companies? An Analysis of Relationships in the Capital Markets	451
JENNIFER L. BROWN and KATHARINE D. DRAKE	
Network Ties Among Low-Tax Firms	483
SABRINA S. CHI, MORTON PINCUS, and SIEW HONG TEOH	
Mispricing of Book-Tax Differences and the Trading Behavior of Short Sellers and Insiders	511
JONGWOON (WILLIE) CHOI	
Can Offering a Signing Bonus Motivate Effort? Experimental Evidence of the Moderating Effects of Labor Market Competition	545
GUS DE FRANCO, FLORIN P. VASVARI, DUSHYANTKUMAR VYAS, and REGINA WITTENBERG-MOERMAN	
Debt Analysts' Views of Debt-Equity Conflicts of Interest	571
JERE R. FRANCIS, MATTHEW L. PINNUCK, and OLENA WATANABE	
Auditor Style and Financial Statement Comparability	605
MICHAEL D. KIMBROUGH and ISABEL YANYAN WANG	
Are Seemingly Self-Serving Attributions in Earnings Press Releases Plausible? Empirical Evidence	635
YANIV KONCHITCHKI and PANOS N. PATATOUKAS	
Taking the Pulse of the Real Economy Using Financial Statement Analysis: Implications for Macro Forecasting and Stock Valuation	669
ELLA MAE MATSUMURA, RACHNA PRAKASH, and SANDRA C. VERA-MUÑOZ	
Firm-Value Effects of Carbon Emissions and Carbon Disclosures	695
KENNETH J. MERKLEY	
Narrative Disclosure and Earnings Performance: Evidence from R&D Disclosures	725
NEMIT SHROFF, RODRIGO S. VERDI, and GWEN YU	
Information Environment and the Investment Decisions of Multinational Corporations ...	759

BOOK REVIEWS, Stephen A. Zeff, editor

Ralf Ewert and Alfred Wagenhofer	
<i>Earnings Management, Conservatism, and Earnings Quality</i>	JONATHAN GLOVER 791
Richard K. Fleischman, Warwick Funnell, and Stephen P. Walker	
<i>Critical Histories of Accounting: Sinister Inscriptions in the Modern Era</i>	JONI J. YOUNG 794
David Hatherly	
<i>The Failure and the Future of Accounting: Strategy, Stakeholders, and Business Value</i>	MICHAEL PAGE 798
Errata	803
Editorial Policy and Style Information	805

Editorial Staff of *The Accounting Review*
2013–2014

SENIOR EDITOR

John Harry Evans III, University of Pittsburgh

EDITORS

Michael L. Ettredge, The University of Kansas

David A. Guenther, University of Oregon

Leslie D. Hodder, Indiana University

Amy P. Hutton, Boston College

Bin Ke, Nanyang Technological University

Lisa Koonce, The University of Texas at Austin

Charles M. C. Lee, Stanford University

Kenneth A. Merchant, University of Southern California

Gregory S. Miller, University of Michigan

Donald V. Moser, University of Pittsburgh

Morton Pincus, University of California, Irvine

Vernon J. Richardson, University of Arkansas

Phillip C. Stocken, Dartmouth College

Beverly R. Walther, Northwestern University

EDITORIAL ASSISTANT

Stacy L. Hoffman, University of Pittsburgh

RESEARCH ASSISTANTS

Jordan M. Bable, University of Pittsburgh

Eric W. Chan, University of Pittsburgh

Michele L. Frank, University of Pittsburgh

American Accounting Association

EXECUTIVE DIRECTOR

Tracey E. Sutherland

PUBLICATIONS DEPARTMENT

Diane Hazard

Lisa Habblitz

Lindsay Wharton Peters

Jan Kovarik (subcontracted)

Copyediting Team (subcontracted)

OTHER AAA STAFF

Julie Smith David

Kathy Casper

Joe Parisi

Erlinda Jones

Debora Gardner

Stephanie Glaser

Kellie Gouwens

Beverly Harrelson

Peggy Turczyn

Barbara Brady

David Boynton

Lorrie Cozzens

Kurt Gardner

Barbara Gutierrez

Deirdre Harris

Nancy Maciag

Pat Stein

Jane Stewart

Dave Frazier

Lisa Hawkins

Ruth Sklar

Editorial Advisory and Review Board

Christopher P. Agoglia, University of Massachusetts

Anwer S. Ahmed, Texas A&M University

Ashiq Ali, The University of Texas at Dallas

Shannon W. Anderson, University of California, Davis

Christopher S. Armstrong, University of Pennsylvania

Anil Arya, The Ohio State University

Benjamin C. Ayers, The University of Georgia

William R. Baber, Georgetown University

Stephen P. Baginski, The University of Georgia

Ramji Balakrishnan, The University of Iowa

Tim Baldenius, New York University

Linda Smith Bamber, The University of Georgia

Orie E. Barron, The Pennsylvania State University

Jan Barton, Emory University

Anne Beatty, The Ohio State University

Jean C. Bedard, Bentley University

Bruce K. Billings, Florida State University

Robert J. Bloomfield, Cornell University

Jennifer L. Blouin, University of Pennsylvania

Mark T. Bradshaw, Boston College

Lawrence D. Brown, Temple University

Brian J. Bushee, University of Pennsylvania

Carolyn M. Callahan, University of Louisville

Jeffrey L. Callen, University of Toronto

Joseph V. Carcello, The University of Tennessee

Mary Ellen Carter, Boston College

Paul K. Chaney, Vanderbilt University

Clara Xiaoling Chen, University of Illinois at Urbana-Champaign

Qiang Cheng, Singapore Management University

Jong-Hag Choi, Seoul National University

Wai-Fong Chua, The University of New South Wales

Michael B. Clement, The University of Texas at Austin

Daniel A. Cohen, The University of Texas at Dallas

William M. Cready, The University of Texas at Dallas

Patricia M. Dechow, University of California, Berkeley

Mark L. DeFond, University of Southern California

Hemang A. Desai, Southern Methodist University

Aiysha Dey, University of Minnesota

Shane S. Dikolli, Duke University

Sunil Dutta, University of California, Berkeley

Scott D. Dyreng, Duke University

Christine E. Earley, Providence College

W. Brooke Elliott, University of Illinois at Urbana-Champaign

Patricia M. Fairfield, Georgetown University

David B. Farber, The University of Texas at El Paso

Mei Feng, University of Pittsburgh

Paul E. Fischer, University of Pennsylvania

Jennifer Francis, Duke University

Jere R. Francis, University of Missouri
Mary Margaret Frank, University of Virginia
James R. Frederickson, Melbourne Business School
Frank B. Gigler, University of Minnesota
Cristi A. Gleason, The University of Iowa
Steven M. Glover, Brigham Young University
Luzi Hail, University of Pennsylvania
Jeffrey W. Hales, Georgia Institute of Technology
Jacqueline S. Hammersley, The University of Georgia
R. Lynn Hannan, Tulane University
Gilles Hilary, INSEAD
Frank D. Hodge, University of Washington
Vicky B. Hoffman, University of Pittsburgh
Chris E. Hogan, Michigan State University
Ole-Kristian Hope, University of Toronto
Richard W. Houston, The University of Alabama
Mingyi Y. Hung, University of Southern California
Yuhchang Hwang, Arizona State University
Raffi J. Indjekjian, University of Michigan
Christopher Ittner, University of Pennsylvania
Debra C. Jeter, Vanderbilt University
Karla M. Johnstone, University of Wisconsin–Madison
S. Jane Jollineau, University of San Diego
Kathryn Kadous, Emory University
Sharon P. Katz, Columbia University
Inder K. Khurana, University of Missouri
Michael D. Kimbrough, University of Maryland
Kenneth J. Klassen, University of Waterloo
April Klein, New York University
Robert W. Knechel, University of Florida
Susan D. Krische, American University
Jagan Krishnan, Temple University
Jayanthi Krishnan, Temple University
Ranjani Krishnan, Michigan State University
Xi (Jason) Kuang, Georgia Institute of Technology
William N. Lanen, University of Michigan
Volker Laux, The University of Texas at Austin
Reuven Leavy, University of Michigan
Andrew J. Leone, University of Miami
Carolyn B. Levine, Rutgers University
Robert Libby, Cornell University
Marlys Gascho Lipe, The University of Oklahoma
Joshua Livnat, New York University
Thomas J. Lopez, The University of Alabama
Henock Louis, The Pennsylvania State University
Joan L. Luft, Michigan State University
Russell J. Lundholm, University of British Columbia
Anne M. Magro, George Mason University
Carol A. Marquardt, Baruch College–CUNY
Michal Matějka, Arizona State University
Dawn A. Matsumoto, University of Washington
Edward L. Maydew, The University of North Carolina at Chapel Hill
Brian W. Mayhew, University of Wisconsin–Madison
John M. McInnis, The University of Texas at Austin
Sarah E. McVay, University of Washington
Lillian F. Mills, The University of Texas at Austin
Brian Mittendorf, The Ohio State University
Partha S. Mohanram, University of Toronto
Steven Monahan, INSEAD
Richard M. Morton, Florida State University
Uday S. Murthy, University of South Florida
James N. Myers, University of Arkansas
Linda A. Myers, University of Arkansas
Venky Nagar, University of Michigan
Terry L. Neal, The University of Tennessee
Karen K. Nelson, Rice University
Jeffrey Ng, Singapore Management University
Per M. Olsson, Duke University
Linda M. Parsons, The University of Alabama
Mark E. Peecher, University of Illinois at Urbana–Champaign
Sean A. Peffer, University of Kentucky
Kathy R. Petroni, Michigan State University
Marlene A. Plumlee, The University of Utah
Suresh Radhakrishnan, The University of Texas at Dallas
K. Raghunandan, Florida International University
Sonja Olhoft Rego, Indiana University Bloomington
Scott Richardson, London Business School
Edward Riedl, Boston University
John R. Robinson, The University of Texas at Austin
Darren T. Roulstone, The Ohio State University
Brian R. Rountree, Rice University
Richard C. Sansing, Dartmouth College
Richard E. Saouma, University of California, Los Angeles
Katherine Schipper, Duke University
Jason D. Schloetzer, Georgetown University
Susan W. Scholz, The University of Kansas
Karen L. Sedatole, Michigan State University
Catherine Shakespeare, University of Michigan
Shiva Sivaramakrishnan, Rice University
J. Reed Smith, Indiana University–Indianapolis
Mark T. Soliman, University of Washington
K. R. Subramanyam, University of Southern California
Hun-Tong Tan, Nanyang Technological University
William B. Tayler, Brigham Young University
Siew Hong Teoh, University of California, Irvine
Kristy L. Towry, Emory University
Senyo Y. Tse, Texas A&M University
Jennifer Wu Tucker, University of Florida
Irem Tuna, London Business School
Andrew Van Buskirk, The Ohio State University
Wim A. Van der Stede, London School of Economics and Political Science
Florin P. Vasvari, London Business School
Mohan Venkatachalam, Duke University
Isabel Yanyan Wang, Michigan State University
Joseph P. Weber, Massachusetts Institute of Technology
Michael Welker, Queen's University
Michael S. Wilkins, Trinity University
Michael G. Williamson, The University of Texas at Austin
Richard H. Willis, Vanderbilt University
Ryan J. Wilson, The University of Iowa
Robert J. Yetman, University of California, Davis
Teri Lombardi Yohn, Indiana University Bloomington
Gwen Yu, Harvard University
Mark F. Zimbelman, Brigham Young University

PAST EDITORS

1926–1929	William A. Paton	1976–1978	Don T. DeCoster
1929–1943	Eric L. Kohler	1978–1983	Stephen A. Zeff
1944–1947	A. C. Littleton	1983–1987	Gary L. Sundem
1948–1949	Robert L. Dixon	1987–1990	William Kinney
1950–1959	Frank P. Smith	1990–1994	A. Rashad Abdel-khalik
1959–1962	Robert K. Mautz	1994–1997	Robert P. Magee
1962–1965	Lawrence L. Vance	1997–2000	Gerald L. Salamon
1965–1967	Wendell Trumbull	2000–2002	Linda Smith Bamber
1968–1970	Charles H. Griffin	2002–2005	Terry Shevlin
1971–1972	Eldon S. Hendriksen	2005–2008	Dan S. Dhaliwal
1973–1975	Thomas F. Keller	2008–2011	Steven J. Kachelmeier

The Impact of CEO Compensation on Nonprofit Donations

Steven Balsam
Temple University

Erica E. Harris
Rutgers, The State University of New Jersey, Camden

ABSTRACT: In this paper we show that supporters reduce donations to nonprofits subsequent to disclosure of high executive compensation. We find evidence consistent with large, sophisticated donors actively seeking out and reacting to compensation information made available in IRS Form 990, while smaller donors react to compensation disclosures in the media. Additional analysis indicates that these results vary systematically across nonprofits, as we find a stronger negative relation in nonprofits classified as more charitable, and a weaker relation in nonprofits that provide services to their donors. In contrast neither grantors nor patrons appear to react to executive compensation disclosures.

Keywords: *executive compensation; nonprofit organizations; donations.*

Data Availability: *All data are available from public sources.*

The CEO of a charity organization should NOT be earning close to a million dollars a year, no matter how big the charity. If you want to get rich, go work for a corporation. SHAME ON YOU, RED CROSS!!! I have donated in the past, naively believing that such a well-known organization would use my donation properly. I won't be donating again.

—Donor review of the American Red Cross
posted on CharityNavigator.org, November 11, 2011

We thank John Harry Evans III (senior editor), two anonymous reviewers, William Baber, Sudipta Basu, Larry Brown, Pamela Engelberg, Elizabeth Gordon, Theresa Gordon, Qianhua Ling, Daniel Neely, Christine Petrovits, Eric Press, Daniel Tinkelman, and Michelle Yetman for their constructive comments. We are also grateful for comments from participants at the 2011 American Accounting Association Mid-Atlantic Regional Meeting and 2012 American Accounting Association Annual Meeting.

Editor's note: Accepted by John Harry Evans III.

Submitted: December 2011
Accepted: September 2013
Published Online: October 2013

I. INTRODUCTION

Media attention surrounding high executive pay has not only increased scrutiny of for-profit firms, but of nonprofits as well (New York State Attorney General 2007; Boroff 2008; Banjo 2009; Wilhelm 2009; Gershman 2011). In times of strained budgets, contributors are especially cautious of supporting nonprofits with high executive compensation (Charity Navigator 2010). Specifically, donors have indicated that high executive salaries have made them “reconsider their donations” (Emerson 2010). In this study we examine the impact of disclosure of nonprofit executive compensation on nonprofit revenues, finding that donors reduce their contributions in response to the disclosure of executive compensation, with the reduction increasing with the amount of compensation reported. However, the donor response to compensation disclosures is not uniform. While all donors seem to react to executive compensation that is disclosed in the print media, only sophisticated donors appear to react to compensation disclosed in Internal Revenue Service (IRS) Form 990. We attribute this heterogeneous result to the fact that for small donors it is not cost effective to seek out information about executive compensation, and many may not even know where to find it (Tinkelman 1998; M. Yetman and R. Yetman 2013).¹ In contrast, large, more sophisticated donors have both the incentive and the ability to acquire executive compensation information.

There are approximately 1.4 million nonprofit organizations in the U.S. that, in aggregate, received donations of over \$260 billion in 2009 (*Nonprofit Almanac* 2011). Understanding the determinants of those donations is essential because the ability to attract and maintain supporters is vital to the survival of these organizations. This study is timely given the recent focus on nonprofit governance, which has included increased mandated disclosures via Form 990, as well as efforts to assure donors that their contributions are being used wisely. With respect to compensation, in their 2005 report to Congress and the nonprofit sector, the Panel on the Nonprofit Sector (2005) included Section 11 entitled “Executive Compensation,” which includes specific requirements for reporting how nonprofit executive compensation is determined.²

Using a sample of more than 5,000 nonprofit firm-year observations, we first examine donor response to executive compensation disclosed in Form 990. We do not find an overall relationship between compensation and donations when compensation is simply disclosed in the Form 990 without being highlighted by the media, but we do find a negative relationship at nonprofits with more sophisticated donors. That is, following Yetman and Yetman (2013) who suggest that sophisticated donors are more likely to place permanent or temporary restrictions on their donations, we find that at organizations that report more restricted donations, Form 990 executive compensation disclosures are inversely related to subsequent public support. In particular, in these organizations we observe a decrease of approximately 3 percent in direct donations growth for each \$100,000 increase in executive compensation.

However, given that the majority of donations are made by small, less sophisticated donors, we expand our analysis to consider executive compensation made public in the print media. Using an event study type analysis, we find that the average donor reacts negatively to newspaper articles discussing high executive compensation. Specifically, we collect articles from the popular press that

¹ In contrast to corporate filings that are available on the Securities and Exchange Commission website, Form 990 is not available on the website of any governmental entity. Guidestar.org provides general access to IRS data, but requires users to register to gain access to Form 990 and tries to sell users value-added services in addition to providing access to the form.

² The Panel on the Nonprofit Sector is “an independent effort by charities and foundations to ensure that the nonprofit community remains a vibrant and healthy part of American society. Formed by Independent Sector in October 2004 at the encouragement of the U.S. Senate Finance Committee, the Panel is comprised of 24 nonprofit and philanthropic leaders whose organizations encompass great diversity in location, mission, perspective, and scope of work” (<http://www.nonprofitpanel.org/about/Index.html>).

discuss nonprofit executive compensation, and find that organizations mentioned in those articles experience 15 percent less growth in direct donations over the two-year window surrounding the year of publication, with the decrease in donations increasing with the level of executive compensation. We further observe that these findings vary systematically with the type of nonprofit. In nonprofits that are more charitable in nature, where the donor and beneficiary differ, there is a stronger negative reaction than in nonprofits that are more service oriented, where the donor receives services from the nonprofit. Explicitly, we find that organizations classified as charitable experience 25 percent less growth in direct donations over the two-year window surrounding the year of publication, while organizations classified as service oriented experience 12 percent less growth.

This paper contributes to accounting research literature on both nonprofits and executive compensation, and is especially timely given the recent focus on nonprofit governance. Section II provides background information and discusses the motivation for the paper. Section III describes the model specification, Section IV discusses the sample selection, and Section V presents the empirical results. Additional analysis and conclusions are presented in Sections VI and VII, respectively.

II. BACKGROUND AND MOTIVATION

Background

Prior literature documents that donations to nonprofit organizations are associated with three primary accounting measures: efficiency, effectiveness, and the ability to continue operations into the future (Parsons 2007; Parsons and Trussel 2008). For example, researchers have extensively explored donors' response to perceived efficiency, specifically the ratio of program expenses to total expenses, referred to as the program ratio (Weisbrod and Dominguez 1986; Posnett and Sandler 1989; Tinkelman 1999; Greenlee and Brown 1999; Okten and Weisbrod 2000; Parsons 2003; Tinkelman 2004; Tinkelman and Mankaney 2007). However, the literature has not examined the impact of executive compensation on contributors' willingness to support nonprofit organizations. That is, while prior literature has examined the determinants of executive compensation in nonprofit organizations (Werner and Gemeinhardt 1995; Oster 1998; Handy and Katz 1998; Baber, Daniel, and Roberts 2002), no paper has looked at whether there is an impact of executive compensation on the funding of nonprofit organizations.³

Impact of Disclosure of Executive Compensation on Donations

Anecdotal evidence suggests that executive compensation impacts donor contributions. For example, the disclosure in 1992 that United Way President William Aramony received an annual compensation package worth \$463,000 received much attention in the media. Even though Aramony resigned from his position shortly after this disclosure, it was estimated that United Way collections were down 10 percent in 1992 from the year earlier, with the implication that some, if not the entire drop, was caused by the scandal (Stodghill, Del Valle, Sandler, and Therrien 1992). Similarly, Parsons, Pryor, and Roberts (2012) suggest that nonprofit managers are concerned that

³ Oster (1998) is the closest study to ours. She examines the impact of the percentage of income derived from private donations on executive compensation, finding a negative relation between the two variables; i.e., compensation decreases as the percentage of income received from private donations increases. The biggest difference between our study and hers is that she explains executive compensation while we explain donations. Because Oster's (1998) study aims to understand the determinants of nonprofit executive compensation, using donations as the explanatory variable is appropriate. Our research questions seek to understand donor behaviors; consequently, we use donations as the response variable.

high administrative costs, which include executive compensation, can reduce the donations and grants that their organizations receive.

Nonetheless, nonprofits compete with other organizations for employee and executive talent. While they may attract individuals who are less driven by extrinsic rewards (Handy and Katz 1998), in most cases they need to provide pay packages that are competitive with individuals' other job opportunities. Consequently, to attract and retain an individual capable of running a multi-million- or billion-dollar enterprise, nonprofits need to provide a compensation package that is competitive with that of other nonprofit or for-profit entities. Emerson (2010) lists the 15 most highly compensated nonprofit executives, excluding colleges and universities, showing that each received in excess of \$800,000 in 2008, with Zarin Mehta of the New York Philharmonic being the top-earner, at more than \$2 million.

While these amounts are substantially less than those paid to for-profit executives (Balsam 2007), the amounts surprise some donors who, according to Emerson (2010, 1), "assume that charity leaders work for free or minimal pay and are shocked to see that they earn six figure salaries." In his discussion of the role of nonprofits, Hansmann (1980) argues that individuals are more likely to give to a nonprofit due to the "nondistribution constraint"; i.e., there are no stockholders to withdraw profits from the firm. High compensation may be viewed by donors as violating the nondistribution constraint. Similarly, Glaser (1994) cites a United Way study on donors in which the top reasons for contributing to a particular charity are trust and making sure the money is well spent. High executive compensation might strike a donor wrong on both accounts, as money paid for compensation may be perceived as not going directly toward the mission of the organization.

The IRS stipulates that nonprofit organizations pay executive compensation that is "fair and reasonable" (Code §4958).⁴ In the absence of a recognized standard for what is fair and reasonable, balancing the market value of executive talent while maintaining the donating public's trust is a critical component of nonprofit success. As a result, we expect that nonprofits perceived as paying their executives excessively will suffer costs in the form of reduced donations, which leads to our first research question:

RQ1: Do donors react negatively to executive compensation disclosures?

Donor Type and Reaction to Executive Compensation Disclosure

We posit that the reaction to executive compensation disclosure will vary with the incentives and sophistication of the donor(s). Nonprofit executive compensation is disclosed in Form 990, a technical and sometimes lengthy filing required by the IRS, containing financial as well as compensation information. While large donors have both the incentive and expertise to find and interpret Form 990 information, smaller donors may not. We argue that small donors are less likely to seek out financial information, as it is simply not cost effective for them to do so. This discussion parallels that in the for-profit literature on investor sophistication. Institutions, often referred to as sophisticated investors, because of their larger holdings and enhanced expertise, are assumed to spend more on monitoring management. Consistent with this increased level of monitoring, research has shown that in for-profit firms with higher levels of institutional shareholdings, the market reacts to information earlier (Balsam, Bartov, and Marquardt 2002), there is less earnings management (Chung, Firth, and Kim 2002), and executive compensation is not only lower, but also more closely tied to firm performance

⁴ There may also be penalties for excess compensation applied by the IRS. Penalties range from fines levied on both executives and approving boards to rare instances of revocation of tax exempt status. In a 2007 IRS report, the Service reported over \$4 million in fines arising from excessive executive pay at charity organizations (Internal Revenue Service 2007).

(Hartzell and Starks 2003). Consequently, we expect larger, more sophisticated donors to gather compensation information via Form 990, while smaller donors will not do so.

Following Yetman and Yetman (2013), we use the existence of restricted donations as a proxy for donor sophistication. We expect only large, sophisticated donors to attach restrictions to their donations, because small donors generally lack the leverage necessary to impose such restrictions on their contributions. We also expect that these large donors will be more likely to expend the resources necessary to obtain information important to their donation decision, such as CEO compensation. These arguments culminate in our second research question:

RQ2: Does the relation between executive compensation disclosure and donations vary with donor sophistication?

Nonprofit Type and Reaction to Executive Compensation Disclosure

We posit that the relationship between compensation disclosure and donations varies with the type of nonprofit organization. Gordon and Khumawala (1999) develop a model of individual giving where the demand for financial reporting increases as the donor's direct involvement with the recipient organization decreases. This demand increases because donors without direct involvement with the organization have fewer alternative sources of information on the performance of the organization. In contrast, supporters who are recipients of nonprofit services can directly evaluate the quality of those services and, thus, have less demand for financial reporting and executive compensation information. For ease of exposition, we refer to these two types of nonprofits as *Service Oriented* and *Charitable*.

In *Service Oriented* nonprofit organizations the donor actually receives or received services from the nonprofit (Hansmann 1980). For example, if the nonprofit is an art museum, then donors are likely to be patrons of the museum. Similarly, if the nonprofit is a university, then donors are likely to be alumni. For both of these entities, donations above and beyond the price paid for services, i.e., program service revenue, are likely to be related to the donor's degree of satisfaction with the services received.

In the case of *Charitable* organizations, a separation exists between the donor and recipient of the service. For example, if the nonprofit is a food bank or soup kitchen, then the donor is unlikely to be a recipient of such services. In these organizations, the donor is less likely to personally evaluate the services provided. Such donors are likely to be more sensitive to the efficiency of the nonprofit and to object to these charities paying high executive compensation. As a result, we expect that supporters of *Charitable* organizations will be more sensitive to executive compensation disclosures than supporters of *Service Oriented* nonprofits. This leads to our third research question:

RQ3: Does the relation between disclosure of executive compensation and donations vary with the type of nonprofit?

Impact of Disclosure of Executive Compensation on Government Grants and Program Service Revenue

The three main sources of revenues for nonprofits are direct donations, government grants, and program service revenue.⁵ Consequently, in addition to our main analysis, which tests the response

⁵ Government grants are funding received from local, state, and federal government agencies, while program service revenues are received for providing services to constituents.

of direct donations to disclosure of executive compensation, we also consider the impact of disclosure of executive compensation on government grants and program service revenue.

We are interested in government grants, given the scrutiny placed on nonprofits receiving grants through government institutions. That is, we expect executive compensation to be reviewed for appropriateness prior to the governmental entity making a grant.⁶ A 2011 New York Times article (Buettner 2011) details a task force set up by the state of New York to investigate compensation paid at nonprofit organizations receiving state funding. Although the state cannot require nonprofit organizations to provide the compensation information requested, organizations that fail to comply could be subject to funding cuts. Moreover, New York Governor Cuomo states that “the special benefits conferred on nonprofit organizations obligated them to keep executive compensation at levels that did not undermine their responsibilities to the public” (Buettner 2011, 1). In sum, we posit that government grantors are interested in executive compensation to avoid making grants to charities that have the potential to embarrass them in the political sphere.

In contrast, we expect a smaller or even zero impact of executive compensation on program service revenue. Program service revenue has been described as “consumptive philanthropy” (Gordon and Khumawala 1999). That is, program service revenue involves a direct exchange between the patron and the nonprofit, allowing the nonprofit to build trust and provide consumers with the ability to evaluate the nonprofit based on nonfinancial measures, such as the quality of services provided, rather than compensation information. This leads to our final research questions.

RQ4: Are government grants sensitive to executive compensation disclosures?

RQ5: Are program service revenues sensitive to executive compensation disclosures?

III. MODEL SPECIFICATION

To test our conjectures empirically, we begin with the following model to examine the impact of disclosure of nonprofit executive compensation on donations:

$$\begin{aligned} \% \Delta \text{Direct Donations}_{(t-1 \text{ to } t+1)} = & \beta_0 + \beta_1 \text{Executive Compensation}_t \text{ or Media Disclosure}_t \\ & + \beta_2 \% \Delta \text{Program Ratio}_{(t-2 \text{ to } t)} \\ & + \beta_3 \% \Delta \text{Fundraising Expenses}_{(t-2 \text{ to } t)} \\ & + \beta_4 \% \Delta \text{Total Assets}_{(t-2 \text{ to } t)} \\ & + \beta_5 \% \Delta \text{Government Grants}_{(t-2 \text{ to } t)} \\ & + \beta_6 \% \Delta \text{Program Service Revenue}_{(t-2 \text{ to } t)} \\ & + \beta_7 \% \Delta \text{Other Revenue}_{(t-2 \text{ to } t)} + \beta_{8-11} \text{Year fixed effects} \\ & + \beta_{12-16} \text{Industry fixed effects} + \varepsilon, \end{aligned} \tag{1}$$

where we define our variables below and measure them using percentage changes over a two-year window. We use a two-year window to ensure that donors have the ability to react to the disclosure, as the Form 990 is filed after the end of the year in question, often more than six

⁶ While not in effect during our sample period, and not specifically putting limits on executive compensation, the federal government has recently increased executive compensation disclosure requirements for grant recipients. See for example the “Guidance for Federal Spending Transparency: Subaward and Executive Compensation Data Reporting Requirements for FFATA Implementation” dated August 27, 2010 and available at https://www.frs.gov/documents/OMB_Guidance_on_FFATA_Subaward_and_Executive_Compensation_Reporting_08272010.pdf

months into the current fiscal year, and the article discussing the nonprofits' executive compensation can appear any time during the year.⁷

In our initial set of tests we first examine the impact of Form 990 disclosure of executive compensation and then of media disclosure of executive compensation, on the percentage change in donations after controlling for other factors found to influence donations. The former is a straightforward examination of the association between the amount of executive compensation and the percentage change in revenues. In contrast, the latter approach is analogous to that used in Schipper and Thompson (1983) and Pownall, Wasley, and Waymire (1993), who model stock returns using determinants of stock returns plus the event(s) under consideration, and Core, Guay, and Larcker (2008) and Kuhnman and Niessen (2012), who look at the impact on CEO compensation in for-profits of media disclosures of executive compensation. This event study measures the reaction to an external shock. To the extent that the shock, in our case the newspaper article, is unanticipated, it is likely to cause a reaction within the donor community. Consequently, we are more likely to be able to observe a causal relationship in our media disclosure event study analysis.

Dependent Variables

We initially focus on *Direct Donations* because we expect this source of funding to be more sensitive to high compensation than *Government Grants* and *Program Service Revenue*.⁸ However, as discussed above, for completeness we also explore the effect of executive compensation on *Government Grants* and *Program Service Revenue*. As noted above and shown in model (1), our dependent variables are measured using percentage changes over a two-year window surrounding the compensation disclosure.

Test Variables

An appropriate test variable should capture the mechanism by which donors, government grantors, and others learn about nonprofit executive compensation. Do they look at Form 990 disclosures, or wait for a scandal, as in the United Way case mentioned above? Because most donations are relatively small, averaging \$91 in 2010 according to Charity Navigator,⁹ many donors may not even be aware of the Form 990 disclosures and, if they are, then they are unlikely to invest the time and effort to review the form. However, if a nonprofit's executive compensation is discussed in the press, then donors are more likely to see it. That is, donors may seek out information on nonprofit executive compensation directly, or they may only find out and react to nonprofit executive compensation information when disclosed by an information intermediary such as the media. Without prior literature to provide guidance, we use both the level of compensation as reported in the Form 990, as well as the existence of media disclosure of executive compensation as test variables. Our test variables are therefore *Compensation from Form 990*, the actual compensation for the highest paid nonprofit executive disclosed in Form 990; and *Media Disclosure*, an indicator variable equal to 1 for years in which a nonprofit is identified as having its executive compensation reported in the news.

⁷ Compensation is reported in Form 990 that is due on the 15th day of the fifth month following the end of the nonprofit's fiscal year.

⁸ Direct public support has been defined by the IRS in the instructions to Form 990 as the "amounts of contributions, gifts, grants, and bequests that the organization received directly from the public."

⁹ See <http://www.charitynavigator.org/index.cfm?bay=content.view&cpid=519>

Control Variables

Prior research (Weisbrod and Dominguez 1986; Posnett and Sandler 1989; Callen 1994; Tinkelman 1998; Khumawala and Gordon 1997; Parsons 2003; Tinkelman and Mankaney 2007; Parsons and Trussel 2008; Tinkelman 2009) confirms that organizational efficiency is associated with the ability to attract donations. We use the ratio of program service expenses to total expenses, or the *Program Ratio*, to measure organizational efficiency. Given that nonprofit organizations are required to categorize their expenses into administrative, fundraising, and program service, the larger the percentage of expenses going to program services, the more efficient the charity is perceived to be. Our expectation, based on prior research, is that a more efficient organization will be better able to attract donations; hence, we expect a positive coefficient on this variable.

Information quantity represents the amount of information displayed to potential contributors, typically in the form of advertising or other means of publicizing the mission of a nonprofit organization. We use the *Fundraising Expenses* of an organization to proxy for the amount of information made available to donors.¹⁰ Weisbrod and Dominguez (1986) and Tinkelman (1999) find that *Fundraising Expenses* are positively related to donations, therefore we expect a positive coefficient on this variable. Prior research has also found a positive relation between nonprofit size and support (Tinkelman 1998; Krishnan and Schauer 2000). As a result, we include year-end *Total Assets* to control for nonprofit firm size and expect it to be positively associated with donations.

Several studies propose that donors refrain from making donations to organizations that receive high levels of *Government Grants*, *Program Service Revenue*, or *Other Revenue* because such support “crowds out” donations (Weisbrod and Dominguez 1986; Posnett and Sandler 1989; Callen 1994; M. Yetman and R. Yetman 2009) as donors may feel that the nonprofits’ financial needs are being met from other sources.¹¹ However, other literature has documented a “crowding-in” effect (Okten and Weisbrod 2000; Petrovits, Shakespeare, and Shih 2011; Yetman and Yetman 2013) as donors may feel more comfortable contributing to a nonprofit that has other sources of revenue, as well as an organization perceived to be monitored by grantors and patrons. As such, we include these three variables to control for the presence of income sources considered either to be substitutes or complements by the donor population.¹² Given the mixed results of prior literature, we do not make sign predictions for these variables.

As with our dependent variables, we measure our control variables as a two-year percentage change. However, following prior literature (e.g., Parsons and Trussel 2008; Petrovits et al. 2011), the control variables are measured with a one-year lag. That is, we measure the percentage change from two years before the article is published or the Form 990 is filed (year $t-2$ in event time) to the year the article is published or the Form 990 is filed (year t in event time).

Given that nonprofit revenues are affected by factors correlated with industry and year, such as the state of the general economy, we incorporate fixed effects to control for industry and year throughout our analyses. Additionally, given that our sample takes the form of a panel dataset, we cluster the standard errors by EIN (employer identification number) throughout our analyses, to adjust for residual correlation among observations belonging to the same nonprofit organization. We further rule out the possibility of correlations in the error terms by employing the Huber-White correction (Huber 1967; White 1980), finding similar inferences, i.e., the sign on our test variables are unchanged and the significance levels are comparable, in untabulated results.

¹⁰ Krishnan, M. Yetman, and R. Yetman (2006) document pervasive expense misreporting in the category of fundraising expenses. In untabulated results we find consistent results when we exclude this variable.

¹¹ “Other Revenue” includes, but is not limited to membership dues and assessments, interest on savings and temporary cash investments, and dividends and interest from securities.

¹² We only include *Government Grants* and *Program Service Revenue* once in the model, consequently when we use that variable as dependent variable we do not include it as an independent variable.

Donor Sophistication

Our theory suggests that large, sophisticated donors will react differently to disclosure of executive compensation than small donors. To measure donor sophistication we first partition sample organizations by the existence of restricted donations to the organization using a 0/1 dummy variable. Following prior nonprofit literature (Yetman and Yetman 2013), restricted donations are gifts made to an organization that have either permanent or temporary limitations placed on them with respect to the timing and/or purpose of their use. Given that approximately 90 percent of the nonprofits in our sample have some donations with restrictions, we also partition our sample by the median of the ratio of restricted donations to total donations.

Service Oriented versus Charitable Organizations

To categorize our sample into organizations that are *Service Oriented* versus *Charitable*, we examine the source of their income. We consider organizations that generate a higher proportion of their revenue from program services to be *Service Oriented*, while those that rely more heavily on other forms of revenues, such as public support, are considered *Charitable*. To test the impact of *Service Oriented* versus *Charitable* orientation on our results, we partition our sample based on the median of program service revenue reliance, measured as program service revenue divided by total revenue, where firms with higher relative program service revenue are assumed to be more *Service Oriented*.¹³

IV. SAMPLE SELECTION

To construct our sample, we begin with the Statistics of Income (SOI) dataset maintained by the National Council on Charitable Statistics (NCCS). This dataset includes financial information for all nonprofit organizations with total assets greater than \$50 million, plus a stratified random sample of smaller organizations across a seven-year period from 2002 to 2008 for a total of 132,106 firm-year observations. While the SOI does not include every nonprofit organization for every year, it does reflect “over 90 percent of all nonprofit revenues,” according to Yetman and Yetman (2013, 1049).

As described above, the dependent variable is the percentage change in revenue in the form of direct donations, grants, or program service revenue from the year before to the year after the disclosure of executive compensation. We exclude year t from our dependent variable computations because it includes a combination of donations made before and after the filing of the Form 990 or media disclosure of executive compensation.¹⁴ The independent variables are also measured over a two-year window and, following prior literature, they are measured with a one-year lag relative to our dependent variable. As a result, because of leads and lags in our model, we require four consecutive years of data for each firm-year observation. Therefore, the first “event” year is 2004, where 2002 and 2003 are years $t-2$ and $t-1$ for 2004 observations, respectively, and the last “event” year is 2007 where 2008 is year $t+1$ for 2007 observations. This requirement limits our sample size to 5,766 firm-year observations.¹⁵

¹³ As an example, our measure of program service revenue reliance classifies the Connie Maxwell Children’s Home as *Charitable* where program services generated 12 percent of their revenue in 2007, whereas the Glimmerglass Opera is classified as *Service Oriented* where program services generated 100 percent of their revenue in 2007.

¹⁴ In addition, although Form 990 is filed or the article is published on a particular date in year t , its effect may not be fully recognized in year t because it may take time for the information to circulate throughout the donor community.

¹⁵ The cause for the drop in sample size from 132,106 to 5,766 is twofold. First, as described above, because we require two years pre- and one year post- “event,” we lose three of our seven years of data. Second, the stratified sampling used by the NCCS for firms with assets of \$50 million or less results in different organizations being included in the database from one year to the next and makes it less likely they will have four consecutive years of data. A consequence of using this dataset is that our sample includes a disproportionate proportion of large nonprofits, and, hence, our findings are not necessarily generalizable to the broader population.

TABLE 1
Sample Selection

Firm-year observations available for analysis from NCCS Statistics of Income databases from 2002–2008	132,106
Less: Firm-year observations omitted from analysis due to lack of time-series information	–126,340
Less: Firm-year observations that have direct donations less than or equal to zero	–238
Firm-year observations used in our media disclosure analysis (1,545 unique organizations)	5,528
Less: Firm-years for which executive compensation could not be obtained	–461
Firm-year observations used in our Form 990 executive compensation disclosure analysis (1,516 unique organizations)	5,067

Feng, Ling, Neely, and Roberts (2011) discuss potential pitfalls of working with nonprofit data, specifically the SOI database. Of particular concern are organizations for which direct donations are less than or equal to zero.¹⁶ Therefore, to minimize the possibility of data errors influencing our results, we remove the 238 firm-year observations for which direct donations are less than or equal to 0 throughout our analysis.¹⁷ As detailed in Table 1, this reduces our sample to 5,528 firm-year observations for 1,545 unique nonprofit organizations. We use this sample for our examination of the impact of media disclosure. For these firm-year observations we then conduct a keyword search of the LexisNexis Academic¹⁸ and Westlaw Campus Research¹⁹ archival services to identify news articles from both national and local publications from 2004 to 2007. We searched these services using key words that included the name of the nonprofit organization and either “compensation” or “pay.” We reproduce a sample article in Appendix A where the search terms are identified in bold. This search yielded a total of 215 articles discussing executive compensation for 173 unique organization years in our sample: 137 organization years had one article, 31 had two articles, four had three articles, and one had four articles.²⁰

We obtain executive compensation information from the Charity Navigator database, which we supplement with hand-collected data from Form 990 filings. Data from Charity Navigator

¹⁶ It is more plausible that grants or program service revenue could be zero.

¹⁷ Additionally, we re-evaluate our models after eliminating observations that have total expenses, total assets, fundraising expenses, or executive compensation that are less than or equal to zero. Deleting these observations does not change our inferences, i.e., the sign on our test variables are unchanged and the significance levels are comparable, but does decrease our sample size, therefore our main analysis only excludes observations for which direct donations are less than or equal to zero.

¹⁸ “LexisNexis Academic helps students and faculty members at colleges and universities save valuable research time by offering them quick, easy access to more than 10,000 of the most credible business, legal, and news sources available in a single location. Full text of articles from more than 2,500 newspapers, including top-tier news publications such as *The New York Times* and *The Washington Post* and local, national, and international sources” (<http://www.lexisnexis.com>).

¹⁹ “Westlaw Campus Research covers thousands of publications, most of which are available in full text. It is especially useful for legal research, as well as for current news coverage and business. In terms of news articles, *Westlaw* contains full text of newspapers including: *The New York Times*, *Boston Globe*, *Atlanta Journal and Constitution*; magazines such as *Newsweek*, *Consumer Reports*, and *Publisher’s Weekly*; over 400 newswires; newsletters; and over 900 broadcast transcripts” (<http://www.westlaw.com>).

²⁰ Following the for-profit literature (e.g., DeFusco, Johnson, and Zorn 1990) we reviewed the articles to ascertain whether other information disclosed herein could also cause a decrease in direct donations, i.e., to see if the articles were “contaminated.” We identified six articles that contained information pertaining to lack of budgetary approval, theft, tax evasion, and illegal drug use, any of which could also lead to a drop in donations. Consequently, we reran our analysis dropping these observations and find that our results are unchanged.

TABLE 2
Sample Distribution

Panel A: Industry Distribution

Industry Group	Frequency	%	Article	%
Arts, culture, and humanities	1,133	20%	50	24%
Education (including higher education)	1,199	22%	92	42%
Health (including hospitals)	419	8%	29	14%
Human services	929	17%	15	7%
Other industries (environment, international, mutual benefit, public and societal benefit, religion)	1,848	33%	29	13%
Total	5,528	100%	215	100%

Panel B: Year Distribution

Year	Frequency	%	Article	% ^a
2004	1,333	24%	48	23%
2005	1,379	25%	51	24%
2006	1,402	25%	69	33%
2007	1,414	26%	47	21%
Total	5,528	100%	215	100%

^a This column sums to 101% due to rounding.

comes directly from Form 990. We then merge these data with financial information obtained from the SOI, which yields a final sample of 5,067 firm-year observations in our Form 990 compensation sample.

Table 2 provides the industry and year distributions for the media disclosure sample, including frequencies for our *Media Disclosure* test variable. As shown in Panel A of Table 2, the classification “Other industries,” which consists of Environment, International, Mutual benefit, Public and Societal Benefit, and Religion, makes up the largest classification at 33 percent of our sample, followed by Education (22 percent), Arts, Culture, and Humanities (20 percent), Human Services (17 percent), and Health (8 percent). In contrast, the largest number of *Media Disclosure* observations are from Education (42 percent) and the fewest (7 percent) from Human Services. This parallels our untabulated observation that mean (median) compensation varies by industry—ranging from a high of \$301,855 (\$259,051) in Education to a low of \$170,566 (\$141,202) in Human Services. As shown in Panel B, our sample is fairly evenly distributed between the four sample years from 2004 to 2007, with 2006 having the largest percentage of events (33 percent), followed by 2005 (24 percent), 2004 (23 percent), and 2007 (21 percent).

V. RESULTS

Descriptive Statistics

Table 3 provides descriptive statistics for the dependent and independent variables in our model. All variables have been winsorized at the 1 and 99 percent levels to mitigate the effects of extreme observations on the results of our analyses. The mean (median) of direct donations is \$28.4 million (\$8.4 million) and the mean (median) percentage change in *Direct Donations* is 30 percent

TABLE 3
Descriptive Statistics

	n	Mean	Median	Std. Deviation	Min.	Max.
Direct Donations _{t+1}	5,528	28,400,000	8,400,659	61,600,000	407,263	431,000,000
%ΔDirect Donations _{t+1}	5,528	0.30	0.04	1.00	-0.85	6.29
Government Grants _t	5,528	9,728,805	104,545	39,500,000	0	291,000,000
%ΔGovernment Grants _t	5,528	0.20	0.00	1.28	-1.00	9.80
Program Service Revenue _t	5,528	35,300,000	1,249,442	143,000,000	0	1,150,000,000
%ΔProgram Service Revenue _t	5,528	0.18	0.03	0.71	-1.00	5.12
Other Revenue _t	5,528	1,174,743	41,455	4,430,922	-125,625	35,500,000
%ΔOther Revenue _t	5,528	1.01	0.00	5.39	-1.87	43.44
Compensation from Form 990 _t	5,067	230,261	200,000	140,864	10,250	628,642
Compensation from Article _t	204	368,759	389,608	130,798	54,500	492,846
Media Disclosure _t	5,528	0.03	0.00	0.16	0.00	1.00
Program Ratio _t	5,528	0.81	0.82	0.10	0.48	0.98
%ΔProgram Ratio _t	5,528	0.02	0.00	0.10	-0.21	0.58
Fundraising Expenses _t	5,528	2,836,533	955,139	5,666,429	22,038	36,900,000
%ΔFundraising Expenses _t	5,528	0.22	0.11	0.55	-0.66	3.37
Total Assets _t	5,528	306,000,000	68,100,000	915,000,000	1,213,570	7,110,000,000
%ΔTotal Assets _t	5,528	0.19	0.13	0.30	-0.28	1.75

Variable Definitions:
%ΔDirect Donations_{t+1} = (direct donations_{t+1} - direct donations_{t-1})/direct donations_{t-1};
%ΔGovernment Grants_t = (government grants_t - government grants_{t-2})/government grants_{t-2};
%ΔProgram Service Revenue_t = (program service revenue_t - program service revenue_{t-2})/program service revenue_{t-2};
%ΔOther Revenue_t = (other revenue_t - other revenue_{t-2})/other revenue_{t-2};
Compensation from Form 990_t = dollar amount of executive compensation collected from Charity Navigator/IRS Form 990;
Compensation from Article_t = dollar amount of executive compensation collected from the media article discussing executive compensation;
Media Disclosure_t = 1 for organizations that have a media article discussing executive compensation;
%ΔProgram Ratio_t = (program ratio_t - program ratio_{t-2})/program ratio_{t-2};
%ΔFundraising Expenses_t = (fundraising expenses_t - fundraising expenses_{t-2})/fundraising expenses_{t-2}; and
%ΔTotal Assets_t = (total assets_t - total assets_{t-2})/total assets_{t-2}.

(4 percent). This is larger than the mean (median) for government grants of \$9.7 million (\$0.1 million), and less (greater) than the mean (median) for program service revenues of \$35.3 (\$1.2) million. The sample mean (median) of compensation as reported in Form 990 is slightly more than \$230,000 (\$200,000), which is significantly less than compensation reported in the media, which is on average, almost \$370,000 (\$390,000).

We examine, but do not tabulate, the correlations among the independent variables. Consistent with prior literature, size (*Total Assets*) is significantly correlated with the majority of the other control variables. For example, the highest correlation is the Spearman correlation of 0.36 between *Total Assets* and *Program Service Revenue*. However, while there are some significant correlations, the relatively low magnitudes of those correlations, as well as the Variance Inflation Factors, all of which are all less than 1, suggest that multicollinearity is not a problem.

Empirical Results

Impact of Executive Compensation Disclosure on Direct Donations

Table 4 presents multivariate results for our Form 990 and *Media Disclosure* test variables. The first column, which utilizes compensation as disclosed in the Form 990, does not find an association between that amount and the percentage change in direct donations. In contrast, the second column, using *Media Disclosure*, reports a strong and statistically significant negative association. This finding supports our conjecture that nonprofit organizations whose executive compensation attracts media attention receive lower subsequent direct donations. The coefficient of -0.147 indicates that the percentage change in direct donations from year $t-1$ to year $t+1$ is reduced by approximately 15 percent when an article appears in year t .

To triangulate our results, we extend our analysis in the third column of Table 4 to test the impact of the magnitude of executive compensation disclosed in the media. Thus, in addition to our *Media Disclosure* test variable, we also analyze the amount of compensation publicized in the articles, finding that *Compensation from Article* is negatively and significantly associated with the change in direct donations. These results suggest that even though compensation information is readily available, donors only react significantly when this information is disseminated by the press. The coefficient of -0.047 indicates that the percentage change in direct donations from year $t-1$ to year $t+1$ is reduced by approximately 5 percent for every \$100,000 of executive compensation disclosed by the media. This is consistent with prior studies in the for-profit literature, which document that stock prices react to subsequent media dissemination of information already publicly available.²¹ Each of the models is highly statistically significant despite an adjusted R^2 of slightly less than 1 percent, in part reflecting the two-period change specification used for all variables.²²

Turning to our control variables, we find significant positive correlations between our response variable and the percentage change in *Program Ratio*, *Fundraising Expenses*, as well as *Government Grants*, although only the first column results are statistically significant for government grants. All of these relations are consistent with those in prior literature. We also find negative and significant relations with the change in *Total Assets*, and the change in *Program Service Revenue*. While the latter is consistent with crowding-out, the negative coefficient on the

²¹ An example is Stice (1991), who finds the market reacts to *Wall Street Journal* earnings announcements even though the information in those earnings announcements was contained in previously filed 10-Ks and 10-Qs. Also see Foster (1979), who finds that the market reacts to criticisms of publicly disclosed accounting practices that were published in *Barron's* by Abraham Briloff.

²² Kothari and Zimmerman (1995) find that in a market setting, returns studies, which employ percentage change variables, have lower R^2 statistics than price models, which utilize levels variables.

TABLE 4
Disclosure Regression Results

Dependent Variable: <i>%ΔDirect Donations_{t+1}</i>	Coefficient p-value	Coefficient p-value	Coefficient p-value
Constant	0.344*** 0.000	0.308*** 0.000	0.307*** 0.000
<i>Compensation from Form 990_t</i>	-0.008 0.387		
<i>Media Disclosure_t</i>		-0.147*** 0.003	
<i>Compensation from Article_t</i>			-0.047*** 0.000
<i>%ΔProgram Ratio_t</i>	0.298* 0.067	0.367** 0.023	0.362** 0.025
<i>%ΔFundraising Expenses_t</i>	0.105*** 0.001	0.091*** 0.002	0.091*** 0.002
<i>%ΔTotal Assets_t</i>	-0.167*** 0.000	-0.152*** 0.000	-0.152*** 0.000
<i>%ΔGovernment Grants_t</i>	0.024* 0.086	0.018 0.144	0.018 0.141
<i>%ΔProgram Service Revenue_t</i>	-0.037** 0.037	-0.044** 0.012	-0.044** 0.012
<i>%ΔOther Revenue_t</i>	-0.002 0.171	0.000 0.830	0.000 0.826
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
n	5,067	5,528	5,528
<i>Media Disclosure</i> n	NA	173	173
Adjusted R ²	0.0093	0.0087	0.0087
Model p-value	0.000***	0.000***	0.000***
Standard errors clustered by EIN	Yes	Yes	Yes

*, **, *** Significant at 10 percent, 5 percent, and 1 percent levels, respectively.

Variable Definitions:

%ΔDirect Donations_{t+1} = (direct donations_{t+1} – direct donations_{t-1})/direct donations_{t-1};
Compensation from Form 990_t = compensation in hundreds of thousands collected from Charity Navigator/IRS Form 990;
Media Disclosure_t = 1 for organizations that have a media article discussing executive compensation;
Compensation from Article_t = compensation in hundreds of thousands collected from the media article discussing executive compensation;
%ΔProgram Ratio_t = (program ratio_t – program ratio_{t-2})/program ratio_{t-2};
%ΔFundraising Expenses_t = (fundraising expenses_t – fundraising expenses_{t-2})/fundraising expenses_{t-2};
%ΔTotal Assets_t = (total assets_t – total assets_{t-2})/total assets_{t-2};
%ΔGovernment Grants_t = (government grants_t – government grants_{t-2})/government grants_{t-2};
%ΔProgram Service Revenue_t = (program service revenue_t – program service revenue_{t-2})/program service revenue_{t-2};
and
%ΔOther Revenue_t = (other revenue_t – other revenue_{t-2})/other revenue_{t-2}.

change in *Total Assets* is surprising. This may be because model variables are specified in percentage change terms, and we are therefore actually capturing firm growth, and not size. When we replace the change specification with log level total assets, the coefficient is positive and significantly related to direct donations, as expected.

Impact of Donor Sophistication on the Relation between Form 990 Disclosure of Executive Compensation and Direct Donations

Although the results in Table 4 indicate that donors in general do not react to Form 990 compensation, certain donors may nevertheless use such information. While it may not be cost effective for small donors to access and incorporate information from the Form 990, larger, more sophisticated donors may have the resources and expertise to do so. To examine the differential effect of donor sophistication on the use of Form 990 compensation information, we partition our sample in two ways. We first partition on the existence of restricted donations, and then partition based on the median ratio of restricted donations to total donations. In both cases our implicit assumption is that sophisticated donors are those who typically place restrictions on their donations. Consequently, we assume that organizations with more donor restrictions have more sophisticated donors.

Table 5 presents the results for our sample partitioned by donor sophistication. In the first two columns, we partition based on the existence of restricted donations, finding support for our conjecture that more sophisticated donors, as proxied by nonprofits with restricted donations, utilize Form 990 compensation in their donation decision. In particular, the results in the second column report a significant negative association between Form 990 executive compensation and the change in direct donations for nonprofits with restricted donations. The estimated magnitude of the effect is a 2 percent decrease in direct donation growth for every \$100,000 increase in compensation. Next, to control for the possibility that this result is driven by the disproportionate sample size, i.e., approximately 90 percent of our observations have some restricted donations, we rerun our analysis after partitioning the sample on the median of the ratio of restricted to total donations. The results in columns 3 and 4 support our expectation that sophisticated donors respond to Form 990, as reflected in the significant negative coefficient on Form 990 executive compensation in column 4 for organizations with restricted donations above the sample median. In particular, we observe a decrease of approximately 3 percent in direct donation growth for every \$100,000 increase in compensation. The findings in this subsection are consistent with larger, more sophisticated donors utilizing Form 990 compensation information when making their donation decisions.²³

Impact of Nonprofit Orientation on the Relation between Media Disclosure of Executive Compensation and Direct Donations

To examine the differential effect of media disclosures concerning executive compensation on *Charitable* versus *Service Oriented* nonprofits, we partition our sample based on program service revenue reliance. As discussed earlier and consistent with prior literature in the area, we expect organizations that are more reliant on program service revenue and, therefore, have a higher ratio of program service revenue to total revenue, to be more *Service Oriented*, while firms with a lower percentage of program service revenue to total revenue to be more reliant on donor support and, therefore, more *Charitable* in nature. We expect nonprofits that are more (less) charitable will pay

²³ Yetman and Yetman (2013) find that all donor types are sensitive to nonprofit organizations reporting zero fundraising expenses on the Form 990, while our results indicate that only large sophisticated donors are using Form 990 information. We believe at least two reasons exist for why small, unsophisticated donors may react to zero fundraising expenses and not compensation information on Form 990. First, fundraising expenses are presented on page 1, in part I of Form 990, while compensation information is disclosed in part V of the pre-2008 Form 990 from which our sample is drawn. It may be that smaller donors are unwilling to dig deeper into the form, where compensation information is disclosed. Second, unsophisticated donors can easily identify zero fundraising expenses, whereas determining if executive compensation is reasonable is a more difficult and subjective task requiring knowledge of the size, industry, and effectiveness of the organization. Analogous to our finding, Yetman and Yetman (2013) also find that smaller donors do not react to their more complex measures of accounting quality.

TABLE 5
Form 990 Compensation Analyses by Donor Sophistication

Dependent Variable:	Observations Without Restricted Donations Coefficient p-value	Observations With Restricted Donations Coefficient p-value	Restricted/Total Donations Below Sample Median Coefficient p-value	Restricted/Total Donations Above Sample Median Coefficient p-value
%ΔDirect Donations _{t+1}				
Constant	0.087 0.637	0.449*** 0.000	0.286*** 0.002	0.539*** 0.000
Compensation from Form 990 _t	0.108 0.128	−0.020** 0.034	0.009 0.690	−0.027** 0.021
%ΔProgram Ratio _t	−0.125 0.710	0.341* 0.053	0.350 0.128	0.341 0.126
%ΔFundraising Expenses _t	0.082 0.322	0.103*** 0.002	0.106** 0.010	0.105** 0.024
%ΔTotal Assets _t	−0.137 0.190	−0.183*** 0.000	−0.113** 0.030	−0.289*** 0.000
%ΔGovernment Grants _t	−0.006 0.865	0.026* 0.079	−0.005 0.708	0.058** 0.033
%ΔProgram Service Revenue _t	−0.009 0.877	−0.039** 0.034	−0.047** 0.035	−0.019 0.490
%ΔOther Revenue _t	0.000 0.980	−0.003 0.134	0.001 0.618	−0.006** 0.010
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
n	423	4,644	2,516	2,551
Adjusted R ²	0.0432	0.0109	0.0107	0.0205
Model p-value	0.0000***	0.000***	0.000***	0.000***
Standard errors clustered by EIN	Yes	Yes	Yes	Yes

*, **, *** Significant at 10 percent, 5 percent, and 1 percent levels, respectively.

Variable Definitions:
%ΔDirect Donations_{t+1} = (direct donations_{t+1} − direct donations_{t−1})/direct donations_{t−1};
Compensation from Form 990_t = compensation in hundreds of thousands collected from Charity Navigator/IRS Form 990;
%ΔProgram Ratio_t = (program ratio_t − program ratio_{t−2})/program ratio_{t−2};
%ΔFundraising Expenses_t = (fundraising expenses_t − fundraising expenses_{t−2})/fundraising expenses_{t−2};
%ΔTotal Assets_t = (total assets_t − total assets_{t−2})/total assets_{t−2};
%ΔGovernment Grants_t = (government grants_t − government grants_{t−2})/government grants_{t−2};
%ΔProgram Service Revenue_t = (program service revenue_t − program service revenue_{t−2})/program service revenue_{t−2};
and
%ΔOther Revenue_t = (other revenue_t − other revenue_{t−2})/other revenue_{t−2}.

lower (higher) compensation and that donations will be more (less) responsive to that compensation. Untabulated results indicate that mean executive salary does in fact vary across these partitions. For nonprofits below the sample median ratio of program services to total revenue; i.e., the more *Charitable*, mean executive compensation is \$206,811; whereas for nonprofits above the sample median, i.e., the more *Service Oriented*, the corresponding mean is \$271,891, a difference that is both economically and statistically significant.

Table 6 reports the results for our partition of nonprofits based on the ratio of program service revenue to total revenue.²⁴ The first two columns of Table 6 report results for the full sample examining the relative impact of *Media Disclosure* on *Charitable* versus *Service Oriented* nonprofits, whereas in columns three and four we use our sample of nonprofits with above median restricted donations to examine the relative impact of *Compensation from Form 990* on *Charitable* versus *Service Oriented* nonprofits with sophisticated donors. In each regression the model is highly significant with an adjusted R^2 ranging from just above 1 to a little over 3 percent.

Organizations in the first (second) regression are those that are more *Charitable* (*Service Oriented*) in nature. Results for both regressions suggest that donors hold back donations to nonprofits that have *Media Disclosure* of executive compensation, although the magnitude, -0.25 versus -0.12 , is significantly greater for *Charitable* nonprofits. In terms of economic implications, for the most charitable organizations the existence of an article on its executive compensation leads to a 25 percent lower percentage change in direct donations from year $t-1$ to year $t+1$. This negative impact decreases to 12 percent for organizations that are more *Service Oriented*.

Similarly, organizations in the third (fourth) regression are those that are more *Charitable* (*Service Oriented*) in nature for our subset of organizations with sophisticated donors (i.e., restricted donations above sample median). In both regressions we observe that the growth in donations is inversely related to *Compensation from Form 990* with the absolute magnitude of 0.034 versus 0.028, being as expected, significantly greater, for *Charitable* nonprofits. In terms of economic implications, for the most charitable organizations' an increase of \$100,000 in executive compensation leads to a 3.4 percent lower percentage change in direct donations from year $t-1$ to year $t+1$, whereas this negative impact decreases to 2.8 percent for organizations that are more *Service Oriented*.

Impact of Executive Compensation Disclosure on Government Grants and Program Service Revenue

Table 7 presents results using government grants and program service revenue as alternative dependent variables. Again, the models are highly significant despite the relatively low adjusted R^2 . Neither the change in government grants nor the change in program service revenue is significantly associated with *Compensation from Form 990*. With respect to *Media Disclosure*, we find a negative and marginally significant ($p = 0.097$) association with the change in government grants, but do not find a significant association with the change in program service revenue. While it is not surprising that individuals receiving goods or services from a nonprofit organization are less concerned about an organization's executive compensation practices, the relatively weak results for government grants is somewhat surprising. One potential explanation is that many grants are multi-year in nature resulting in an even greater lag before the impact on government grants is fully recognized.²⁵

²⁴ In untabulated results we find similar results when we use the percentage of direct donations to total donations, referred to as donation reliance, as an alternative definition of *Charitable* versus *Service Oriented*.

²⁵ To test this possibility, in untabulated analyses we redefine our dependent variable as the change in government grants over first a three-year and then a four-year window. Unfortunately, our sample size drops dramatically, with the sample size using a four-year event window falling to 839 total observations with 36 media disclosure observations. Perhaps because of this low power, the coefficients on *Media Disclosure* and *Compensation from Form 990* continue to be insignificant.

TABLE 6
Sample Partitioned by Program Service Revenue Reliance

Dependent Variable: <i>%ΔDirect Donations_{t+1}</i>	All Nonprofits		Nonprofits with Sophisticated Donors ^a	
	<i>PSR/TR Below Sample Median Charitable Coefficient</i> p-value	<i>PSR/TR Above Sample Median Service Oriented Coefficient</i> p-value	<i>PSR/TR Below Sample Median Charitable Coefficient</i> p-value	<i>PSR/TR Above Sample Median Service Oriented Coefficient</i> p-value
Constant	0.343*** 0.000	0.356*** 0.001	0.474*** 0.000	0.347 0.151
<i>Media Disclosure</i>	-0.254*** 0.004	-0.117** 0.048		
<i>Compensation from Form 990_t</i>			-0.034* 0.069	-0.028* 0.068
<i>%ΔProgram Ratio_t</i>	0.357* 0.082	0.435* 0.089	0.238 0.291	0.706 0.154
<i>%ΔFundraising Expenses_t</i>	0.124*** 0.002	0.053 0.205	0.000*** 0.000	0.000*** 0.000
<i>%ΔTotal Assets_t</i>	-0.050 0.287	-0.323*** 0.000	-0.200** 0.033	-0.342** 0.028
<i>%ΔGovernment Grants_t</i>	0.010 0.507	0.034 0.103	0.055 0.108	0.077* 0.069
<i>%ΔProgram Service Revenue_t</i>	-0.022 0.406	-0.081*** 0.000	0.007 0.845	-0.091** 0.020
<i>%ΔOther Revenue_t</i>	-0.001 0.805	0.000 0.937	-0.007** 0.027	-0.004 0.379
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
n	2,771	2,757	1,275	1,276
<i>Media Disclosure</i> n	40	133	NA	NA
Adjusted R ²	0.0129	0.0197	0.0196	0.0344
Model p-value	0.000***	0.000***	0.000***	0.000***
Standard errors clustered by EIN	Yes	Yes	Yes	Yes

*, **, *** Significant at 10 percent, 5 percent, and 1 percent levels, respectively.
^a Organizations with restricted/total donations above the sample median.

Variable Definitions:
 $\% \Delta \text{Direct Donations}_{t+1} = (\text{direct donations}_{t+1} - \text{direct donations}_{t-1}) / \text{direct donations}_{t-1}$;
 $\text{PSR/TR} = \text{ratio of program service revenue to total revenue}$;
 $\text{Media Disclosure}_t = 1$ for organizations that have a media article discussing executive compensation;
 $\text{Compensation from Form 990}_t = \text{compensation in hundreds of thousands collected from Charity Navigator/IRS Form 990}$;
 $\% \Delta \text{Program Ratio}_t = (\text{program ratio}_t - \text{program ratio}_{t-2}) / \text{program ratio}_{t-2}$;
 $\% \Delta \text{Fundraising Expenses}_t = (\text{fundraising expenses}_t - \text{fundraising expenses}_{t-2}) / \text{fundraising expenses}_{t-2}$;
 $\% \Delta \text{Total Assets}_t = (\text{total assets}_t - \text{total assets}_{t-2}) / \text{total assets}_{t-2}$;
 $\% \Delta \text{Government Grants}_t = (\text{government grants}_t - \text{government grants}_{t-2}) / \text{government grants}_{t-2}$;
 $\% \Delta \text{Program Service Revenue}_t = (\text{program service revenue}_t - \text{program service revenue}_{t-2}) / \text{program service revenue}_{t-2}$;
and
 $\% \Delta \text{Other Revenue}_t = (\text{other revenue}_t - \text{other revenue}_{t-2}) / \text{other revenue}_{t-2}$.

TABLE 7
Government Grants and Program Service Revenue

Dependent Variable	%ΔGovernment Grants _{t+1} Coefficient p-value	%ΔProgram Service Revenues _{t+1} Coefficient p-value	%ΔGovernment Grants _{t+1} Coefficient p-value	%ΔProgram Service Revenues _{t+1} Coefficient p-value
Constant	0.250*** 0.001	0.141*** 0.000	0.263*** 0.000	0.150*** 0.000
Compensation from Form 990 _t	0.004 0.821	0.006 0.478		
Media Disclosure _t			-0.126* 0.097	0.022 0.717
%ΔProgram Ratio _t	-0.109 0.574	0.070 0.647	-0.049 0.785	0.039 0.786
%ΔFundraising Expenses _t	0.028 0.439	0.062** 0.016	0.037 0.291	0.056** 0.024
%ΔTotal Assets _t	0.129 0.138	0.123** 0.024	0.098 0.202	0.111** 0.030
%ΔGovernment Grants _t		-0.014 0.152		-0.011 0.213
%ΔProgram Service Revenue _t	-0.019 0.537		-0.019 0.510	
%ΔOther Revenue _t	0.007 0.235	0.000 0.950	0.007 0.233	0.001 0.601
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
n	5,067	5,067	5,528	5,528
Media Disclosure n	NA	NA	173	173
Adjusted R ²	0.0109	0.0068	0.0100	0.0060
Model p-value	0.000***	0.000***	0.000***	0.000***
Standard errors clustered by EIN	Yes	Yes	Yes	Yes

*, **, *** Significant at 10 percent, 5 percent, and 1 percent levels, respectively.

Variable Definitions:
%ΔGovernment Grants_{t+1} = (government grants_{t+1} – government grants_{t-1})/government grants_{t-1};
%ΔProgram Service Revenue_{t+1} = (program service revenue_{t+1} – program service revenue_{t-1})/program service revenue_{t-1};
Compensation from Form 990_t = compensation in millions collected from Charity Navigator/IRS Form 990;
Media Disclosure_t = 1 for organizations that have a media article discussing executive compensation;
%ΔProgram Ratio_t = (program ratio_t – program ratio_{t-2})/program ratio_{t-2};
%ΔFundraising Expenses_t = (fundraising expenses_t – fundraising expenses_{t-2})/fundraising expenses_{t-2};
%ΔTotal Assets_t = (total assets_t – total assets_{t-2})/total assets_{t-2};
%ΔGovernment Grants_t = (government grants_t – government grants_{t-2})/government grants_{t-2};
%ΔProgram Service Revenue_t = (program service revenue_t – program service revenue_{t-2})/program service revenue_{t-2}; and
%ΔOther Revenue_t = (other revenue_t – other revenue_{t-2})/other revenue_{t-2}.

Summary of Empirical Results

Taken together, Tables 4 through 7 provide evidence that donors respond to executive compensation in nonprofit organizations. We provide evidence consistent with different types of donors responding to different forms of disclosure, with larger, more sophisticated donors

responding to executive compensation as reported in Form 990, whereas smaller donors only respond to executive compensation when reported in the media. We also provide evidence consistent with the response varying by type of organization, with smaller responses to executive compensation in more *Service Oriented* nonprofits than in *Charitable* nonprofits.

Specifically, Table 4 reports a negative relation between *Direct Donations* (column 2) and our *Media Disclosure* test variable, but no results for the Form 990 compensation test variable. Table 5 provides evidence consistent with Form 990 compensation information being used by more sophisticated donors, as proxied by the existence of restricted donations, who presumably have an incentive to seek out compensation information that is too costly for smaller donors to obtain. Table 6 reports results when our sample is partitioned based on nonprofit orientation (*Charitable* versus *Service Oriented*), finding that the sensitivity of direct donations to executive compensation disclosure is more pronounced for *Charitable* nonprofits. Finally, Table 7 tests the impact of disclosure of executive compensation on the two other largest pools of nonprofit revenues: government grants and program service revenue. Here we find a weak association between growth in *Government Grants* and *Media Disclosure* of executive compensation, no association between growth in *Government Grants* and *Compensation from Form 990*, and no association between growth in *Program Service Revenue* and either of our executive compensation test variables.

VI. ADDITIONAL ANALYSIS

To investigate the impact of whether the frequency or type of executive compensation media disclosure impacts donor reaction, Table 8 presents four additional regressions. Column 1 uses as the test variable the number of articles published related to an organizations' executive compensation. The logic for using this variable is that the more articles written about a nonprofits' executive compensation, the greater the impact, via increased dissemination of that information, as generally speaking the articles are in different publications. In this analysis our test variable is defined as a continuous variable between 1 and 4, indicating the number of articles for the organization. Results for this additional measure indicate that the greater the number of articles published highlighting the organizations' executive compensation, the greater the adverse effect on direct donations.

Column 2 provides an analysis of the impact of the publication source. Our reasoning is that readership varies by publication and newspapers with more readership should have a greater impact. For tractability we partition the articles into whether they appeared in national or local newspapers, where an example of the former includes the *Wall Street Journal* with a circulation of 2.1 million (Lauder 2012), while an example of the latter would be the *Pittsburgh Post-Gazette* with circulation of just over 200,000 in 2009 (<http://pgmediakit.com/>).²⁶ Here we include two indicator variables that take the value of 1 if the article appeared in a national or local newspaper, respectively.²⁷ Not surprisingly we find that articles in national publications, with larger readership, have a significant impact on nonprofit donations, while local articles do not. That is, direct donations decrease when compensation information is publicized in a national newspaper, but not when publicized in a local newspaper.

Our final additional analysis relates to the tone of the *Media Disclosure*. We have so far assumed that any mention of executive compensation in the print media is negative. For example, the following phrase is contained in the article in Appendix A: "frustration is building as the

²⁶ Examples of other national sources are *The New York Times*, *Washington Post*, and *USA Today*.

²⁷ As noted in the text, we have 36 organization years where there is more than one article. In 31 of those cases there are articles published in both a national and local newspaper. Consequently, in those situations both indicator variables take the value of 1.

TABLE 8

Additional Analysis

Dependent Variable: <i>%ΔDirect Donations_{t+1}</i>	Coefficient p-value	Coefficient p-value	Coefficient p-value	Coefficient p-value
Constant	0.308*** 0.000	0.308*** 0.000	0.376*** 0.000	0.000
<i>Number of Articles_t</i>	-0.058*** 0.001			
<i>National Article_t</i>		-0.166*** 0.004		
<i>Local Article_t</i>		-0.114 0.220		
<i>Tone_t</i>			-0.108*** 0.012	0.107 0.404
<i>Media Disclosure_t</i>				-0.322*** 0.041
<i>%ΔProgram Ratio_t</i>	0.364** 0.024	0.368** 0.023	1.085*** 0.004	1.085 0.004
<i>%ΔFundraising Expenses_t</i>	0.091*** 0.002	0.091*** 0.002	0.099*** 0.009	0.099*** 0.009
<i>%ΔTotal Assets_t</i>	-0.153*** 0.000	-0.153*** 0.000	-0.162*** 0.004	-0.163*** 0.004
<i>%ΔGovernment Grants_t</i>	0.018 0.140	0.018 0.144	0.058* 0.098	0.058 0.100
<i>%ΔProgram Service Revenue_t</i>	-0.044** 0.012	-0.044** 0.012	-0.095*** 0.003	-0.095*** 0.003
<i>%ΔOther Revenue_t</i>	-0.001 0.817	0.000 0.833	-0.003 0.463	0.003 0.470
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
n	5,528	5,528	5,528	5,528
<i>Media Disclosure</i> n	173	173	173	173
Adjusted R ²	0.0087	0.0088	0.0063	0.0065
Model p-value	0.000***	0.000***	0.000***	0.000***
Standard errors clustered by EIN	Yes	Yes	Yes	Yes

, * Significant at 5 percent and 1 percent levels, respectively.

Variable Definitions:
%ΔDirect Donations_{t+1} = (direct donations_{t+1} – direct donations_{t-1})/direct donations_{t-1};
Number of Articles_t = 1 to 4 depending on how many articles discuss the nonprofits’ executive compensation;
National (Local) Article_t = 1 for organizations that have a national (local) media article discussing executive compensation;
Tone_t = 1 for organizations with a media article discussing executive compensation of neutral tone, 2 for organizations with a media article discussing executive compensation of negative tone;
Media Disclosure_t = 1 for organizations that have a media article discussing executive compensation;
%ΔProgram Ratio_t = (program ratio_t – program ratio_{t-2})/program ratio_{t-2};
%ΔFundraising Expenses_t = (fundraising expenses_t – fundraising expenses_{t-2})/fundraising expenses_{t-2};
%ΔTotal Assets_t = (total assets_t – total assets_{t-2})/total assets_{t-2};
%ΔGovernment Grants_t = (government grants_t – government grants_{t-2})/government grants_{t-2};
%ΔProgram Service Revenue_t = (program service revenue_t – program service revenue_{t-2})/program service revenue_{t-2};
and
%ΔOther Revenue_t = (other revenue_t – other revenue_{t-2})/other revenue_{t-2}.

salaries of orchestra executives and conductors skyrocket” (Tindall 2004, 1). However, there is variability in the tone of the articles. Consequently, in the third and fourth columns of Table 8 we include *Tone* in place of, and in addition to, *Media Disclosure* following the methodology in Loughran and McDonald (2011). In column three we find that when used in place of *Media Disclosure*, *Tone* is negative and significant, indicating that articles that are more negative in tone produce a larger negative impact on donation growth. However when *Tone* is used in the same equation as *Media Disclosure*, as presented in column 4, *Media Disclosure* remains negative and significant, while *Tone* becomes insignificantly different from zero.

VII. CONCLUSIONS

This paper examines the impact of nonprofit executive compensation on donations, finding that donors take executive compensation into account when they decide whether and how much to contribute to a particular charity. Using an event study type approach, with the event being the mention of executive compensation in the print media, we show that organizations mentioned in those articles experience 15 percent less growth in direct donations over a two-year window surrounding the year in which the article is published, with the decrease in donations increasing with the level of compensation reported. While we do not find an overall relation when compensation is simply disclosed in the Form 990 and not highlighted by the media, we do find that in nonprofits with a large amount of restricted donations, the growth in donations declines by approximately 3 percent for each \$100,000 of additional compensation. That is, sophisticated donors appear to seek out and incorporate Form 990 compensation information in their donation decision.

In addition to our main findings, we also find results that suggest that this relation is most negative for nonprofits that are more charitable in their focus, as opposed to nonprofits that provide services to their donors. We also find this decrease is larger if the article is published in a national publication, and when the nonprofits’ compensation is reported more often.

Our overall empirical results, which are robust to the inclusion of variables used in the prior literature to explain direct donations, support the notion that donors reduce their contributions to nonprofits that pay their executives more. We provide a unique look at nonprofit donor reactions to executive compensation, using research methods common to the for-profit capital market literature, but new to nonprofit studies. Our research extends and is incremental to prior work showing that donors consider a charity’s success, efficiency, and stability in their donation decision. It also builds on for-profit research that documents the political costs associated with high executive compensation.

REFERENCES

- Baber, W. R., P. L. Daniel, and A. A. Roberts. 2002. Compensation to managers of charitable organizations: An empirical study of the role of accounting measures of program activities. *The Accounting Review* 77 (3): 679–693.
- Balsam, S. 2007. *Executive Compensation: An Introduction to Practice & Theory*. Available at: <http://www.worldatwork.org/bookstore/html/product.jsp?id=9781579631703>
- Balsam, S., E. Bartov, and C. Marquardt. 2002. Accruals management, investor sophistication, and equity valuation: Evidence from 10-Q filings. *Journal of Accounting Research* 40 (4): 987–1012.
- Banjo, S. 2009. IRS questions pay of charity executives. *Wall Street Journal Online* (February 18).
- Boroff, P. 2008. Big bucks for theater chiefs draw criticism as sign of excess. Available at: <http://www.bloomberg.com/apps/news?pid=newsarchive&sid=aNSaiFBIWL9A&refer=m>
- Buettner, R. 2011. State seeks data on pay of leaders at nonprofits. *The New York Times* (August 25): 1.
- Callen, J. 1994. Money donations, volunteering and organizational efficiency. *The Journal of Productivity Analysis* (5):215–228.

- Charity Navigator. 2010. CEO compensation study. Available at: http://www.charitynavigator.org/__asset__/studies/2010_CEO_Compensation_Study_Revised_Final.pdf (accessed September 15, 2012).
- Chung, R., M. Firth, and J. Kim. 2002. Institutional monitoring and opportunistic earnings management. *Journal of Corporate Finance* 8 (1): 29–48.
- Core, J. E., W. Guary, and D. F. Larcker. 2008. The power of the pen and executive compensation. *Journal of Financial Economics* 88: 1–25.
- DeFusco, R. A., R. R. Johnson, and T. S. Zorn. 1990. The effect of executive stock option plans on stockholders and bondholders. *Journal of Finance* 45: 617–627.
- Emerson, G. 2010. 15 Highest-paid charity CEOs. Available at: <http://www.mainstreet.com/print/19173>
- Feng, N., Q. Ling, D. Neely, and A. Roberts. 2011. *Using Archival Data Sources to Conduct Nonprofit Accounting Research*. Working paper, Suffolk University, Marquette University, University of Wisconsin–Milwaukee, and University of Virginia.
- Foster, G. 1979. Briloff and the capital market. *Journal of Accounting Research* 17 (1): 262–274.
- Gershman, J. 2011. Charity probe questions. *Wall Street Journal* (September 9): A.21.
- Glaser, J. S. 1994. *The United Way Scandal: an Insider's Account of What Went Wrong and Why*. Wiley Nonprofit Law: Finance and Management Series. New York, NY: Wiley.
- Gordon, T. P., and S. B. Khumawala. 1999. The demand for not-for-profit financial statements: A model of individual giving. *Journal of Accounting Literature* (18): 31–56.
- Greenlee, J. S., and K. L. Brown. 1999. The impact of accounting information on contributions to charitable organizations. *Research in Accounting Regulation* 13: 113–128.
- Handy, F., and E. Katz. 1998. The wage differential between nonprofit institutions and corporations: Getting more by paying less? *Journal of Comparative Economics* 26 (2): 246–261.
- Hansmann, H. 1980. The role of nonprofit enterprise. *The Yale Law Journal* 89 (5): 835–901.
- Hartzell, J. C., and L. T. Starks. 2003. Institutional investors and executive compensation. *The Journal of Finance* 58 (6): 2351–2374.
- Huber, P. J. 1967. The behavior of maximum likelihood estimates under nonstandard conditions. In *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability*, Vol. 1, 221–233. Berkeley, CA: University of California Press.
- Internal Revenue Service (IRS). 2007. Report on exempt organizations executive compensation compliance project—Parts I and II. Available at: http://www.irs.gov/pub/irs-tege/exec._comp._final.pdf
- Khumawala, S. B., and T. P. Gordon. 1997. Bridging the credibility of GAAP: Individual donors and the new accounting standards for nonprofit organizations. *Accounting Horizons* 11 (3): 45–68.
- Kothari, S. P., and J. L. Zimmerman. 1995. Price and return models. *Journal of Accounting and Economics* 20 (2): 155–192.
- Krishnan, J., and P. C. Schauer. 2000. The differentiation of quality among auditors: Evidence from the not-for-profit sector. *Auditing: A Journal of Practice & Theory* 19 (2): 9–25.
- Krishnan, R., M. H. Yetman, and R. J. Yetman. 2006. Expense misreporting in nonprofit organizations. *The Accounting Review* 81 (2): 399–420.
- Kuhnan, C. M., and A. Niessen. 2012. Public opinion and executive compensation. *Management Science* 58 (7): 1249–1272.
- Lauder, W. 2012. *Journal* remains biggest U.S. newspaper. Available at: <http://online.wsj.com/article/SB10001424052702304050304577377812281217548.html>
- Loughran, T., and B. McDonald. 2011. When is a liability not a liability? Textual analysis, dictionaries, and 10-Ks. *Journal of Finance* 66: 35–65.
- New York State Attorney General. 2007. Attorney general Cuomo recovers \$1,900,000 in excess compensation from Morris Foundation. May 15. Available at: <http://www.ag.ny.gov/press-release/attorney-general-cuomo-recovers-1900000-excessive-compensation-morris-foundation>
- Nonprofit Almanac, The*. 2011. The nonprofit sector in brief: Public charities, giving, and volunteering. Facts and figures from *The Nonprofit Almanac*. Available at: <http://www.urban.org/UploadedPDF/412434-NonprofitAlmanacBrief2011.pdf> (accessed September 15, 2012).
- Okten, C., and B. Weisbrod. 2000. Determinants of donations in private nonprofit markets. *Journal of Public Economics* 75 (2): 255–272.

- Oster, S. 1998. Executive compensation in the nonprofit sector. *Nonprofit Management and Leadership* 8 (3): 207–221.
- Panel on the Nonprofit Sector. 2005. Strengthening of charitable organizations. Available at: http://www.nonprofitpanel.org/Report/final/Panel_Final_Report.pdf
- Parsons, L. M. 2003. Is accounting information from nonprofit organizations useful to donors? A review of charitable giving and value-relevance. *Journal of Accounting Literature* (22): 104–129.
- Parsons, L. M. 2007. The impact of financial information and voluntary disclosures on contributions to not-for-profit organizations. *Behavioral Research in Accounting* (19): 179–196.
- Parsons, L. M., C. Pryor, and A. A. Roberts. 2012. *The Use of Real Ratio Management or Accounting Discretion to Manage Efficiency Ratios: Evidence from Nonprofit Managers*. Working paper, The University of Alabama, University of Southern Maine, and University of Virginia.
- Parsons, L. M., and J. M. Trussel. 2008. Financial reporting factors affecting donations to charitable organizations. *Advances in Accounting* (23): 263–285.
- Petrovits, C., C. Shakespeare, and A. Shih. 2011. The causes and consequences of internal control problems in nonprofit organizations. *The Accounting Review* 86 (1): 325–357.
- Posnett, J., and T. Sandler. 1989. Demand for charity donations in private non-profit markets. *Journal of Public Economics* 40 (2): 187–200.
- Pownall, G., C. Wasley, and G. Waymire. 1993. The stock price effects of alternative types of management earnings forecasts. *The Accounting Review* 68 (4): 896–912.
- Schipper, K., and R. Thompson. 1983. The impact of merger-related regulations on the shareholders of acquiring firms. *Journal of Accounting Research* 21 (1): 184–221.
- Stice, E. K. 1991. The market reaction to 10-K and 10-Q filings and to subsequent *Wall Street Journal* earnings announcements. *The Accounting Review* 66 (1): 42–55.
- Stodghill, R., C. Del Valle, G. Sandler, and L. Therrien. 1992. United they stand. *Businessweek* (October 19): 40–41.
- Tindall, B. 2004. The plight of the white-tie worker. Available at: <http://www.nytimes.com/2004/07/04/arts/the-plight-of-the-white-tie-worker.html?pagewanted=all&src=pm>
- Tinkelman, D. 1998. Differences in sensitivity of financial statement users to joint cost allocations: The case of nonprofit organizations. *Journal of Accounting, Auditing and Finance* 13 (4): 377–393.
- Tinkelman, D. 1999. Factors affecting the relation between donations to not-for-profit organizations and an efficiency ratio. *Research in Governmental and Nonprofit Accounting* (10): 135–161.
- Tinkelman, D. 2004. Using nonprofit organization-level financial data to infer managers' fundraising strategies. *Journal of Public Economics* 88 (9–10): 2181–2192.
- Tinkelman, D. 2009. Unintended consequences of expense ratio guidelines: The Avon breast cancer walks. *Journal of Accounting and Public Policy* 28 (6): 485–494.
- Tinkelman, D., and K. Mankaney. 2007. When is administrative efficiency associated with charitable donations? *Nonprofit and Voluntary Sector Quarterly* 36 (1): 41–64.
- Weisbrod, B., and N. Dominguez. 1986. Demand for collective goods in private nonprofit markets: Can fundraising expenditures help overcome free-rider behavior? *Journal of Public Economics* 30 (1): 83–95.
- Werner, S., and G. Gemeinhardt. 1995. Nonprofit organizations: What factors determine pay levels? *Compensation and Benefits Review* 27:53–60.
- White, H. 1980. A heteroscedasticity-consistent covariance matrix estimator and a direct test for heteroscedasticity. *Econometrica* 48: 817–830.
- Wilhelm, I. 2009. Executive compensation at charities attracts new scrutiny. *The Chronicle of Philanthropy* (February 18).
- Yetman, M. H., and R. J. Yetman. 2009. Determinants of nonprofits' taxable activities. *Journal of Accounting and Public Policy* 28 (6): 495–509.
- Yetman, M. H., and R. J. Yetman. 2013. Do donors discount low-quality accounting information? *The Accounting Review* 88 (3): 1041–1067.

APPENDIX A

The New York Times—July 4, 2004 Sunday—Late Edition, Final

The Plight of the White-Tie Worker

BYLINE: By BLAIR TINDALL

Blair Tindall, a professional oboist, is writing “Mozart in the Jungle” for Grove/Atlantic Press.

SECTION: Section 2; Column 1; Arts and Leisure Desk; Pg. 1

THIS summer, the backstage chatter at American orchestras is dominated by one subject: money.

In the musicians’ locker rooms, frustration is building as the salaries of orchestra executives and conductors skyrocket, while the players’ salaries stagnate. With contracts about to expire at 16 major orchestras, including such powerhouses as the **New York Philharmonic**, the Chicago Symphony, the Philadelphia Orchestra, and the Cleveland Orchestra, the discontent is rising to a slow boil.

“This year’s negotiation will probably be the most difficult in a number of years,” said Ed Ward, the president of the Chicago local of the American Federation of Musicians. Paralleling trends in corporate pay, salaries for orchestra leaders shot up during the late 1990’s. Among the 18 American orchestras with 52-week contracts, at least 7 pay their music directors more than \$1 million, and 3 pay their managers more than \$700,000.

The **New York Philharmonic** is at the top. In 2003, the most recent year for which tax documents are available to the public, the orchestra paid its music director, Lorin Maazel, \$2.28 million for 14 weeks with the orchestra and an annual tour. Zarin Mehta, then the orchestra’s executive director (and now its president) got \$600,000 and \$150,000 in benefits. (The Philharmonic, like most other orchestras discussed in this article, declined to comment on salaries.)

That would be a lot in any case, but it’s especially striking at a time when classical music finds itself on the ropes. For big American orchestras, audiences are declining, government and private donations have dropped and recording activity has virtually disappeared. Some argue that the surest way out of this apparent crisis is to hire the best executives and conductors, no matter their price. Others say that these high salaries are an unbearable financial burden—and that they reinforce classical music’s image as an elitist, exclusionary world that is increasingly out of touch with its listeners.

The situation is striking at other top orchestras as well. According to tax documents from 2002, the most recent year available in these cases, Mark Volpe, the managing director of the Boston Symphony, was making \$349,923; Gideon Toeplitz, then the managing director of the Pittsburgh Symphony, \$335,984; Henry Fogel, then the president of the Chicago Symphony, \$330,000; and Brent Assink, the executive director of the San Francisco Symphony, \$322,688.

Meanwhile, over the last decade, as pay increases for symphony leaders have soared, the players’ annual raises dropped from 3.9 percent in 1993 to 1.7 percent in 2003, according to the International Conference of Symphony Orchestra Musicians.

Speaking anonymously for fear of reprisals, many musicians say they are tired of being asked to absorb their orchestras’ financial woes while executives use those troubles to exact even higher salaries. “If management starts pleading poverty in this year’s negotiations, it would be very tacky,” said a New York Philharmonic string player.

Between 1997 and 2002, the compensation of Thomas W. Morris, then the executive director of the Cleveland Orchestra, jumped 92 percent, while the musicians received a 19 percent raise for the same period. And Allison Vulgamore, the president of the Atlanta Symphony, won a 64 percent raise between 1999 and 2002, while salaries for Atlanta’s players increased 11 percent.

Ms. Vulgamore had taken pay freezes during 4 of her 11 years with the orchestra, which has become much healthier financially since her arrival. And Mr. Fogel, now the president of the

American Symphony Orchestra League, points out that there may be other unseen reasons for a sharp raise, such as moving allowances, bonuses or parity with other orchestras.

In any case, players are being asked to make concessions. At several orchestras, managements have proposed pay freezes or cuts, according to union officials.

“It’s the pattern in every industry, that executive **compensation** has grown over 30 years, while workers’ pay stayed flat for the same period,” said Robert Frank, a professor of management at Cornell University and the author of “The Winner-Take-All Society.”

But the similarity ends there. In the corporate world, the incentive to trim labor costs is to return greater profits to investors. The classical music business is nonprofit, which means that the investors aren’t looking for financial rewards. They may, however, re-examine the amounts they’re asked to donate to an institution that pays its top people lavishly while simultaneously crying poverty, posting deficits and urging fund-raising campaigns. And like donors, musicians are scrutinizing the salaries as well.

Do Analysts Follow Managers Who Switch Companies? An Analysis of Relationships in the Capital Markets

Francois Brochet
Harvard University

Gregory S. Miller
University of Michigan

Suraj Srinivasan
Harvard University

ABSTRACT: We examine the importance of professional relationships developed between analysts and managers by investigating analyst coverage decisions in the context of CEO and CFO moves between publicly listed firms. We find that top executive moves from an origin firm to a destination firm trigger analysts following the origin firm to initiate coverage of the destination firm in 10 percent of our sample, which is significantly higher than in a matched sample. Analyst-manager “co-migration” is significantly stronger when both firms are within the same industry. Analysts who move with managers to the destination firm exhibit more intense and accurate coverage of the origin firm than they do in other firms and compared to other analysts covering the origin firm. The advantage no longer holds after the executive’s departure, and most of the analysts’ advantage does not carry over to the destination firm. However, the analysts do increase the overall market capitalization of firms in their coverage portfolio. Our results hold after Regulation Fair Disclosure, suggesting that these relationships are not based on selective disclosure. Overall, the evidence shows both the importance and limitations of professional relationships in capital markets.

Keywords: *management turnover; analyst coverage; capital market relationships.*

Data Availability: *Data are publicly available from sources identified in the article.*

We are grateful to Ray Ball, Nerissa Brown, Trevor Harris, Paul Healy, Christian Leuz, Phil Shane, Wayne B. Thomas (editor), Rodrigo Verdi, Joe Weber, two anonymous reviewers, and seminar participants at The University of Chicago, INSEAD, Massachusetts Institute of Technology, Washington University, Yale University, the 2010 AAA Annual Meeting, the 2010 FARS Meeting, the 2010 NYU Summer Camp, and the 2009 HBS IMO Conference for their helpful comments. We thank Jesse Lamarre-Vincent, Zach Borichevsky, and Peter Nore for their help in data collection. Professor Miller thanks the Michael R. and Mary Kay Hallman Faculty Fellow for funding.

Editor’s note: Accepted by Wayne B. Thomas.

Submitted: November 2010
Accepted: September 2013
Published Online: October 2013

I. INTRODUCTION

Capital market participants often argue that relationships among entrepreneurs, capital providers, and information intermediaries are central to a well-functioning marketplace.¹ Yet, there is limited empirical academic research on relationships in financial markets. Further, individual relationships play no role in traditional agency paradigms used to model financial markets. Combined, the current empirical and analytical research provides limited insights into these potentially important aspects of financial markets. In this paper, we study personal relationships in financial markets by focusing on the analyst decision to initiate coverage when a manager in a currently covered firm moves to a new firm. Our goal is to use the important and observable economic event of initiating analyst coverage to identify and then investigate relationships in financial markets. While valid identification of relationships in financial markets is a challenge, the highly visible nature of much of analysts' activities provides an opportunity to construct tests that provide insights into this important and relatively under-studied aspect of capital markets.

We predict and find that analysts are most likely to initiate coverage when the managers were previously at firms that the analysts covered more intensively and accurately, both relative to other firms in that analyst's portfolio and to other analysts following the company. While analysts continue to follow the origin firm, their intensity and accuracy reverts to the mean of the firm once the manager departs. The analyst intensity and accuracy in the manager's new firm quickly becomes average despite it being relatively new to the analyst. Combined, these results show a pattern of coverage decisions that indicate some managers and analysts develop important, ongoing relationships in capital markets.

Developing a valuable relationship requires investment of time and energy by the manager and analysts, presumably with the goal of long-term benefits to the analyst, who better understands the firm, and the manager, who develops a conduit, and often supporter, in the financial markets. The costs in time and energy make it unlikely that all manager/analyst pairings can develop such a deep relationship. Thus, we expect that only a subset of analysts following any firm will develop a valuable relationship with the managers. However, once established, the analysts have incentives to maintain the relationship when managers change firms. These incentives arise from an enhanced understanding of the manager and the ability to gain access to a new firm. Indeed, a growing body of literature shows that managerial styles have a broad impact on the firm operations and its disclosure policies (Bertrand and Schoar 2003; Bamber, Jiang, and Wang 2010; Dyreng, Hanlon, and Maydew 2010; Ge, Matsumoto, and Zhang 2011), suggesting that managerial characteristics are transportable across firms and that having a relationship with a manager who switches firms can benefit analysts who wish to expand their portfolios.²

Our study is based on a large sample of management changes involving CEOs and CFOs between the years 1994 and 2007 for a sample of U.S. firms covered by the ExecuComp database.

¹ The term "relationship" in this paper refers to the connections between capital market participants that are formed due to interactions over time. This is consistent with definitions of relationships in general provided in several common dictionaries. For example, "the mutual dealings, connections, or feelings that exist between two parties, countries, people, etc., a business relationship" (Collins English Dictionary 2003) and "A particular type of connection existing between people related to or having dealings with each other" (American Heritage Dictionary 2004).

² Our predictions are based on prior research, but relative to many other areas of academic inquiry, there is only a limited amount of research in this area. As an alternative, we also discussed the value of relationships with several analysts and managers. Many of the analysts indicated that the value of the relationship was a factor in deciding whether to initiate coverage in a new firm, although they also pointed out the structural constraints to the new coverage. Several managers and investor relations (IR) specialists indicated that they value prior analyst relationships and will actively attempt to attract those analysts when they change firms.

We identify the company the managers leave (origin firm) and the new company they join (destination firm). We then examine whether analysts who followed the origin firm initiate coverage in the destination firm over the first year of the manager's new job, which we refer to as "migrating." By focusing on executives who move across firms, we identify a setting in which we can separate firm- from manager-specific effects. Our control sample consists of matched firms that did not experience a management turnover.

In unconditional tests of the entire sample we find that managers and analysts do co-migrate—analysts initiate coverage in the manager's new firm in over 10 percent of the managerial moves. This is almost three times more frequent than in the control firms, indicating that relationships have an impact on coverage decisions. While the magnitude may appear low, it is consistent not only with the idea that initiation of coverage is costly and analysts are only likely to incur these costs for a relatively valuable relationship, but also with the noise inherent in any indirect measurement of personal relationships. Further, these unconditional tests do not take into account the impact of structural issues that may affect the value of initiation of coverage, including completely precluding it.

We examine firm-level and analyst-level determinants of co-migration. Analysts are generally industry specialists and we expect this to be a primary structural constraint. Consistent with this, we find that analyst migration is much more likely (22.90 percent versus 5.32 percent across industries) when the manager moves within the same industry.³ Using a regression approach we examine several structural factors that may impact the decision to initiate coverage. Industry, measured by SIC or the amount of analyst overlap between the two firms, continues to be a strong predictor of coverage. Analysts are also more likely to move if there is greater potential for benefits such as lower competition, as measured by lower analyst following in the new firm, or greater investor interest, as measured by migration of institutional investors and firm size. Combined, the evidence indicates that relationships impact coverage decisions, but that structural factors influence the ability of analysts to preserve these relationships.

Having examined firm characteristics associated with the likelihood that analysts will co-migrate with managers, we next explore the specific analyst-level factors of the manager/analyst relationship that are likely to impact the co-migration. We recognize these relationships have many facets and use several proxies to capture the quality of the relationship. We compare the analyst activities/outcomes for our origin (destination) firm to the activities in the other firms the analyst follows and to activities/outcomes of other analysts following the origin (destination) firm. We consider an analyst as having a *comparative* advantage when the analyst is more frequent/better with an activity in the origin (destination) firm compared to other firms covered by the same analyst. An analyst has a *competitive* advantage when they are more frequent/better than other analysts that cover the origin (destination) firm. We find evidence consistent with analysts responding to their own comparative advantages, as well as competitive advantages over other analysts.

We find that the likelihood of co-migration is positively related to proxies for frequent interactions with management such as the length of time the analyst has covered the origin firm, the frequency with which the analyst issues forecasts, the number of horizons over which the analyst forecasts, and the likelihood that the analyst issues a report following a one-on-one meeting with management. Combined, the above suggest that co-migrating analysts cover the origin firm more intensely. Co-migrating analysts also exhibit greater forecast accuracy in the origin firm, suggesting that migrating analysts have a better understanding of the migrating managers' operating or

³ Untabulated results also indicate that 85 percent of the analysts who co-migrate with a manager across industries already cover the destination firm's industry, which further emphasizes the importance of industry as a structural barrier to migration.

reporting style. While we expect that migrating analysts would hold a more positive view about the firm due to the better relationship, we do not find evidence to support this prediction nor do we find evidence of migration when prior recommendations have been more profitable.

Next, we examine the consequences of analyst-manager co-migration for the analysts' output in the origin and destination firms.⁴ We find the migrating analysts lose most of their coverage intensity and information advantage in the origin firm once the manager leaves, suggesting that the strength of their coverage in the origin firm is driven by the manager/analyst relationship. While we find little evidence that their advantage is transferred to the destination firm, we do find that the analysts are quickly "average" in the new firm when compared to their own activities in other firms and with other analysts covering the new firm. We also find that analysts are more likely to meet with managers in the new firm and that the analysts' total coverage market capitalization increases, both suggesting that the analysts maintain access advantages that they can help to provide to their institutional customers who are likely to value this access and compensate the analyst's firm accordingly (Groysberg, Healy, and Maber 2011; Bushee, Jung, and Miller 2011).

Finally, in contrast to Cohen, Frazzini, and Malloy (2010), our results are generally consistent across the pre- and post-Regulation Fair Disclosure (Reg FD) time periods, suggesting that the importance of the relationship is not driven by preferential access to material nonpublic information.⁵ This also indicates that the relations we identify are driven by different incentives and mechanisms than those in Cohen et al. (2010). This suggests that multiple factors can influence relationships, indicating a need for more work in the area of capital market relationships in general.

Overall, our results are consistent with the premise that capital market participant/managerial relationships induce some participants to "migrate" with a manager following an executive job change. There is relatively limited work in the area of relationships in financial markets. Early work focused on interactions between two institutions rather than individuals. For example, studies on investment banking relationships and favorable analyst recommendations (e.g., Lin and McNichols 1998; Dechow, Hutton, and Sloan 2000) focused on how the brokerage firm's (one institution) incentives to get investment banking work impacted their coverage of various public companies (a second institution). While those studies show that institutional relationships matter in capital markets, they do not shed light on the role of personal relationships. More recent papers have investigated individual relationships, such as those between senior executives and research analysts, but again with a focus on how these relationships may skew or circumvent normal market functions (Westphal and Clement 2008; Cohen, Frazzini, and Malloy 2008). These relationships seem to have lost their benefits post-Reg FD (Cohen et al. 2010).

In contrast to the preceding studies, our paper examines whether personal relationships developed in the course of repeated interactions in a professional setting between sell-side analysts and corporate executives survive in a setting in which executives move to a new firm. Thus, we expand the understanding of relationships in financial markets beyond those that are based on the firm level or undertaken primarily to circumvent normal market operations. The paper also contributes to the growing literature on social networks by showing a dynamic process of a network adjusting (Brass, Galaskiewicz, Greve, and Tsai 2004), and to our understanding of the impact of managerial reputation on analyst coverage.

⁴ We use the term "output" to indicate externally visible actions or activities that the analysts take in interacting with or reporting on the company.

⁵ Our results are also robust after controlling for the measure of school ties used in Cohen et al. (2010) in the subsample of our firms for which we could obtain them. This provides further evidence that we have identified a unique aspect of capital market relationships.

II. PRIOR LITERATURE

Prior Research on Managerial Relationships with Analysts

The prior research on managerial relationships with analysts has focused on incentives to bias analyst outputs. Early work focused on institutional relationships, generally examining how broker affiliations impact analysts' coverage of the firm. Results suggest that underwriting relationships with companies caused analysts to provide more favorable views or to be slow in reporting bad news (McNichols and O'Brien 1997; Lin and McNichols 1998; Dechow et al. 2000).

A smaller body of more recent research examines personal relationships, but still focuses on relationships that skew analysts or managerial actions. Westphal and Clement (2008) document that analysts provide more favorable coverage when managers have granted them favors.⁶ Granting of favors increases when managers are likely to miss expectations or when they make decisions likely to be viewed negatively by analysts, such as a diversification, suggesting that the favors are an attempt to influence analysts' interpretations of unrelated company news. Further, managers penalize analysts with negative views by reducing favors and curtailing access to management (Westphal and Clement 2008; Mayew 2008).

While some of these relationships arise from professional interactions, they also develop in social settings such as shared professional school affiliations (Cohen et al. 2010). All of this prior research indicates an attempt to derive an advantage through a preferred position, either of the manager or the analyst. Recent regulations, such as Reg FD and the Global Analyst Research Settlement, have attempted to reduce the possibility of such actions, potentially decreasing the value of a personal relationship subsequent to regulation changes. The findings in Cohen et al. (2010) are consistent with such a reduction occurring.

Managerial Style and Enhanced Analysts' Understanding of the Firm

While the above research suggests relationships often skew analysts' coverage of the firm, knowledge of the firm's management may also help the analysts to better cover the firm, enhancing accuracy of forecasts and profitability of recommendations. Building on the upper-echelon theory (Hambrick and Mason 1984), prior papers show that managerial styles impact operating decisions ranging from investment and financial policies (Bertrand and Schoar 2003) to their aggressiveness with tax strategies (Dyreng et al. 2010). Equally important to analysts/investors, managers also have unique financial reporting styles that impact financial communication (DeJong and Ling 2010; Ge et al. 2011), use of forecasts (Bamber et al. 2010; Yang 2012), and aggressiveness with earnings management (Hribar and Yang 2010; Hwang and Kim 2012; Jiang, Petroni and Wang 2010). As analysts build a relationship with managers, they may develop a fuller understanding of the managers' operating and financial reporting styles. That understanding will allow the analyst to evaluate current actions and information more effectively, as well as to predict future firm activities more accurately. Managers have an incentive to ensure that analysts develop this competence and are able to correctly evaluate the firm, since they face career penalties for missing even short-term analyst forecasts (Mergenthaler, Rajgopal, and Srinivasan 2010). In fact, much of the investor relations activities are designed to provide managers with an opportunity to interact directly with analysts and important investors to build this understanding (Bushee and Miller 2012).

⁶ For example, business-related favors such as helping with access to other business people or personal favors such as assisting a child's admission to a prestigious school or sponsoring a club membership.

III. PREDICTIONS

Relationships with Managers and the Initiation of Coverage

The existing literature suggests both legitimate and self-serving incentives for analysts and managers to value their existing relationships. While some relationship benefits may be firm-specific, many appear to be manager-specific. For example, once an understanding of a manager's operating and communication style has been developed, the analyst is likely to predict the performance of any firm in which the manager serves more accurately. Similarly, a manager who has done favors or granted preferred access while at one firm is likely to continue this after he/she changes firms. While these arguments indicate the value of strong relationships, developing such relationships is costly to both the manager and analyst. Further, the benefits come directly from having an "inside" or unique advantage, suggesting they cannot exist between all managers and analysts. Thus, these relationships are likely to develop with only a subset of analyst and manager counterparts. Once analysts have developed such a valuable advantage, they have incentives to continue their relationship with managers following employment changes. This will allow the analyst to continue to reap the benefits of the relationship and provide the enhanced advantage of an increased set of firms the analyst can effectively cover (Groysberg et al. 2011).⁷ Accordingly, we provide the following prediction:

H1: Relative to random firms, analysts are more likely to initiate coverage in firms that have recently hired executives from a firm covered by the analyst.

Determinants of Co-Migration: Firm Characteristics

While we argue that the manager/analyst relationship is valuable, it is clear that structural issues are likely to influence an analyst's ability/desire to initiate coverage of a new firm even if they value their relationship. Thus, when considering the magnitude of initiation and its cross-sectional determinants, we must also consider the potential mitigating impact of these structural barriers. It both reduces our initial expectations of coverage and may skew the cross-sectional variation in firms for which coverage is initiated.

Analyst coverage is normally structured by industry and specialized knowledge is developed along industry lines (Bushee and Miller 2012; Bradshaw, Miller, and Serafeim 2009). Thus, analysts incur lower costs to initiate coverage within an industry and, subsequently, enjoy higher benefits. Further, the analyst's understanding of managerial operating style is also most likely to translate effectively if the manager is staying within the same industry. Based on these decreased costs and increased benefits, we expect analyst initiation of coverage to be more pronounced when a manager moves within an industry. We measure industry using both a standard industry classification and the degree of analyst overlap between the origin and destination firm. Analyst overlap allows us to infer industry groups based on economic similarities between firms (Beatty, Liao, and Yu 2013) that are not captured by conventional industry classifications, e.g., because analysts use their own proprietary industry classification schemes (Bhojraj, Lee, and Oler 2003).

⁷ In addition to the traditional compensation for direct research in a firm, brokerage firms are increasingly being compensated based on the managerial access their analysts are able to provide to large shareholders (Groysberg et al. 2011; Bushee et al., 2012). When the analysts initiate coverage of a new firm, they increase the market capitalization for which they can provide both direct research and access, effectively enhancing their brokerage firm's ability to make money. If the existing relationship reduces the costs and/or enhances the ability to provide either of these services in a broader set of firms, then it effectively creates a monetary benefit to the brokerage firm and analyst.

In addition to the structural issue of industry limitations, analysts are likely to initiate coverage in the new firm if they perceive a greater benefit. Specifically, if a manager moves to a firm with low current analyst coverage, then the analyst would face reduced competition in covering the firm and may be able to capture more benefits, such as providing managerial access to investors. Similarly, analysts may perceive a greater benefit of their relationship if investors from the origin firm also take a position in the new firm. This would indicate that the investors see value in the manager joining the firm, creating a demand for information regarding the manager in the new firm. The analyst can benefit from fulfilling such a demand.

Combined, these predictions lead to our second hypothesis:

H2a: Analysts are more likely to co-migrate if the destination firm: (i) is in the same industry, (ii) has fewer analysts, and (iii) has origin firm investors become investors in the manager's new destination firm.

Determinants of Co-Migration: Analyst Characteristics

While our prior predictions investigate the frequency and determinants of initiating coverage from a structural view, we are also interested in developing an understanding of the role of relationships in impacting the manager/analyst co-migration to a firm. We expect analysts' likelihood of initiating coverage in the destination firm to be increasing in the strength of their relationship with the manager and the benefit they derive from that relationship, subject to more important structural economic factors that drive analyst coverage. From an empirical standpoint, it is difficult to develop valid measures of relationships in most capital market settings. We take advantage of the public aspect of analysts' outputs, such as forecasting, recommendations, etc., and the revealed preferences created by the analyst's decision to initiate/not initiate coverage in the new firm to create a research design that allows us to measure various dimensions of the analyst relationships with management and the potential benefits they accrue from this relationship. Our design allows us to examine these observables prior to the management move using the origin firm, which allows us to develop insight into the existing relationship. We are then able to examine whether the co-migrating analyst's outputs change at the origin firm once the manager departs, and to analyze the quality of the outputs at the destination firm. By using this time-series of analyst outputs we are able to estimate the impact and value of the managerial relationships.⁸

Analysts who cover a firm more intensely have repeated interactions with the manager, providing them many opportunities to develop both an understanding of managerial style and to exchange favors. Thus, we measure coverage intensity as years of coverage, the level of forecasting activity, measured as the number of updates and the number of periods forecasted, and likelihood of having one-on-one meetings with management.⁹ We expect all of these to be positively related to the likelihood that the analysts will initiate coverage in the managers' new firm.

A richer relationship may allow the analyst to provide more accurate forecasts due to a better understanding of the business and managerial style. Additionally, prior research indicates that

⁸ We perform these tests *conditional* on having moved. Thus, we are examining analysts for whom we can observe that the structural constraints were not so binding as to prohibit the move. An alternative design would be to include all analysts/firm combinations in the analyses and simultaneously examine both structural constraints and proxies of the relationships. However, if the structural constraints are binding, then we would effectively be combining two regimes and restricting the analyses to treat them as one. For example, if a move outside of an industry is binding and thus the analyst cannot initiate coverage, then variation in our measures of intensity and accuracy would not matter for that firm. If we pooled those observations with situations where the analysts do have an option of initiating coverage, then it would lead to incorrect inferences.

⁹ We observe only the meetings reported by the analysts that we use as a proxy for all meetings held.

analysts can benefit from preferential access to managers that can lead to informational advantages, enabling the analyst to provide more accurate forecasts (Mayew 2008; Westphal and Clement 2008). Thus, we predict analysts are more likely to initiate coverage in the destination firm when they have been better at forecasting performance in the origin firm and when they have more profitable recommendations. Note that these predictions are based on the belief that analysts value relationships that allow them to perform their job more effectively, rather than stemming from personal gain.¹⁰

We also expect analysts to be more likely to initiate coverage of the new firm if they have provided more positive recommendations of the origin firm. These positive recommendations may be due to the belief that the manager is high quality. In that situation the analyst may believe the high-quality manager has the ability to identify a firm with high potential and is likely to succeed in the new firm, suggesting an opportunity for the analyst to identify a high-potential stock. Furthermore, managers have shown preference in favors and access to analysts who provide higher ratings (Westphal and Clement 2008; Mayew 2008). Analysts may initiate coverage to continue receiving these preferential treatments at the new firm even if they believe the recommendations are overstated.¹¹ Combined, the arguments above lead to our next hypothesis:

H2b: Analysts are more likely to co-migrate if they: (i) covered the origin firm more intensely, (ii) more accurately forecasted performance of the origin firm, and (iii) have provided more favorable recommendations for the origin firm.

Consequences of Co-Migration: Analysts' Output for Origin Firms

Above we have argued that the relationship and related understanding between the manager and analyst have led to more intense interactions, more accurate predictions of earnings and investment profitability, and more favorable recommendations. Following that argument, once the manager leaves the firm these actions and outcomes should revert to a more normal level. If the levels were to remain the same, then it would suggest it is not the manager/analyst relationship driving the observed outcomes. Stated as a formal hypothesis:

H3a: After analysts co-migrate, they: (i) cover the origin firm no more intensely than comparison firms/analysts, (ii) lose their forecasting advantage in the origin firm, and (iii) provide no more favorable recommendations for the origin firm.

Consequences of Co-Migration: Analysts' Output for Destination Firms

Similarly, the strength of the relationship between the manager and the analyst may enable the analyst to exhibit more intense, accurate, and favorable coverage of the manager's new firm. Presumably, the analysts' decision to initiate coverage of the new firm suggests that they expect to benefit from such a move. If the analysts have a relationship-based advantage in covering the old firm, then they will gain an advantage in the new firm as well, subject to several limitations. First, the learning curve when initiating coverage of the new firm may reduce the value of the

¹⁰ Also note that while these advantages may provide independent incentives for an analyst to co-migrate, they also may simply be a result of the more intensive interactions discussed above. That is, to the extent that the analyst activities lead to more accurate/profitable forecasts, we may just be documenting the outcomes of those interactions. While this reduces our *ex ante* ability to validly interpret demonstrated results, our empirical findings discussed later in the paper actually find no relation with profitability of recommendations.

¹¹ Ideally, we would be able to separate the high recommendations into those that indicate a belief in quality management and those that indicate an attempt to curry favor. However, we cannot observe intention.

relationship. In that case, the relationship would provide no advantage relative to analysts already covering those firms or to their own coverage of other firms. Second, the incoming manager also faces a learning curve in the new firm, limiting the effectiveness of the relationship in providing the analyst with superior knowledge about the firm. Finally, to the extent that analysts' understanding was actually driven by the combination of the manager and a specific firm, the analyst will carry over fewer benefits when following the same manager in a new firm. That is, the analyst will lose an advantage in the old firm and not gain one in the new firm. Thus, it becomes somewhat difficult to determine the proper benchmark for evaluating the impact of the relationship on the analyst's actions with the new firm. Despite this uncertainty, we provide the following hypothesis:

H3b: When analysts co-migrate, they: (i) cover the destination firm more intensely, (ii) gain a forecasting advantage in the destination firm, and (iii) provide more favorable recommendations for the destination firm.

We test H2b, H3a, and H3b by comparing the analyst to their actions at other firms they cover (comparative advantage) and by comparing the analyst with other analysts following the origin firm (competitive advantage).

IV. SAMPLE SELECTION AND MATCHING PROCEDURE

Sample Selection

Our study focuses on CEOs, CFOs, COOs, Presidents, and Vice-Presidents migrating between publicly listed companies. Using ExecuComp as a starting point, we identify instances of CEO or CFO turnover over the 1994–2007 period. We select pairs of origin and destination firms by identifying executives who either (1) join an ExecuComp firm as CEO or CFO from a firm in which they held a senior executive position or (2) depart from an ExecuComp firm as CEO or CFO to join another company as a senior executive. We also include migrations where the incoming CEO or CFO is promoted to that position within three years after joining the destination company as a top executive. To identify origin/destination firms, we search the names of the executives joining and leaving ExecuComp firms in the BoardEx database to retrieve their employment history. This enables us to track most executives joining and leaving ExecuComp firms. Although BoardEx started only in 2000, the database is fairly comprehensive in covering the employment history of (1) former executives of companies still listed in the 2000s and (2) individuals still active as executives or directors in the 2000s. Nevertheless, for some of the turnovers that occurred in the 1990s, we also search Factiva and SEC filings to gather information on executives' employment history and turnover dates. We require that at least one analyst cover the origin firm as of the year prior to the executive migration. Analyst coverage is not required in the destination firm prior to the executive move, but both firms must be in the I/B/E/S database. Other variables are obtained from Compustat, CRSP, and the Thomson-Reuters Mutual Fund Holdings database.¹²

¹² One concern is that our results depend on our reliance on ExecuComp to identify at least one of two firms in the origin/destination pairs. Indeed, recent research shows that ExecuComp firms differ from the rest of the population along many characteristics (Cadman, Klasa, and Matsunaga 2010). To alleviate this concern, we collect additional turnover data (1) within the universe of U.S. firms covered by BoardEx but excluding ExecuComp firms and (2) among European companies also covered by BoardEx and I/B/E/S. Untabulated results indicate that the incidence of executive-analyst co-migration is similar to our main sample (10.67 percent for the 75 non-ExecuComp U.S. turnovers, 11.97 percent for the 142 European turnovers) and significantly greater than their respective matched samples. Hence, while our results are based on a sample of large U.S. companies, the co-migration phenomenon exists in other samples.

Table 1 reports summary statistics on the sample selection procedure in Panel A and characteristics of the executive turnovers in our sample in Panel B. As Panel A indicates, we start from a large number of external CEO and CFO turnovers involving at least one ExecuComp firm. However, several data requirements reduce the sample eventually included in our analysis. The sum of CEO and CFO turnover observations (835) exceeds the total number of pairs of firms (829) due to six cases where the CEO and CFO of the destination firm move together from the same origin firm. Panel B shows our full sample where destination firms are, on average, significantly smaller and followed by fewer analysts than origin firms. However, this need not mean that the migrations in our sample are “demotions,” although the median stock return in the origin firms is negative.¹³ Indeed, for strict CEO/CEO or CFO/CFO moves (94 and 325 observations, respectively), origin and destination firms are not significantly different from each other in terms of size and analyst coverage. In contrast, other CEO and CFO turnovers (219 and 197 observations, respectively) occur from origin firms that are substantially larger than the destination firms. These patterns are broadly consistent with prior research (Hayes and Schaefer 1999; Fee and Hadlock 2004).

Matching Procedure

To assess the significance of the executive-analyst co-migration we create a control sample of matched firms constructed to mirror the executives’ move from the origin to the destination firm. That is, we create a matched sample of origin and destination firms using propensity-score-based proximity to the actual origin-destination pair. We create a propensity score for both the origin and destination firm based on coefficient estimates from the following model:

$$\begin{aligned} \text{Pr}(\text{Turnover} = 1) = f(\text{Analyst Following Origin, Analyst Following Destination,} \\ \text{Same Four-Digit SIC, Analyst Overlap, Firm Size Origin,} \\ \text{Firm Size Destination, Book To Market Origin,} \\ \text{Book To Market Destination, Stock Return Origin,} \\ \text{Stock Return Destination, Recent Turnover,} \\ \text{Industry and Time Fixed Effects}). \end{aligned} \tag{1}$$

All variables are defined in Appendix A.

As illustrated in Figure 1, we first estimate model coefficients when the sample consists of the migrating executives’ origin firms paired with all firms in the destination firms’ two-digit SIC group that is step (a) in Figure 1. The dependent variable *Turnover* is equal to 1 if the origin firm is paired with the actual destination firm, and 0 otherwise. Equation (1) thus models the decision of an executive from the origin firm to join a certain destination firm.

Second, we estimate model coefficients when the sample consists of the migrating executives’ destination firms paired with all firms in the origin firms’ two-digit SIC group and covered by at least one analyst in I/B/E/S, step (b) in Figure 1. The dependent variable *Turnover* is equal to 1 if the destination firm is paired with the actual origin firm, and 0 otherwise. In this sample, Equation (1) now models the destination firm’s decision to hire an executive from a certain origin firm.

¹³ Ideally, we would like to identify managers whose departure from the origin firm is not voluntary, since they are less likely to be followed by analysts. However, to the extent that we require departing executives to join another publicly listed company, the incidence of forced turnover should be relatively low in our sample. Nevertheless, we partition the sample based on the origin firm’s stock return in an attempt to separate voluntary from forced departures. Untabulated results show no robust evidence of analyst migration significantly differing across partitions and no evidence that co-migration is more or less common when the executive moves to a lateral or higher position.

TABLE 1
Sample Selection and Descriptive Statistics

Panel A: Sample Selection

Sample Selection Step	Number of Observations	
	CEO	CFO
External turnover (in or out of ExecuComp)	1,891	2,345
Origin and destination firms in Compustat	946	1,311
At least one analyst following the origin firm in I/B/E/S	601	869
Data available for control variables	353	585
Matched on propensity score (with data available for control variables in matched observations)	313	522

Panel B: Firm Characteristics for Origin Firms Compared to Destination Firms

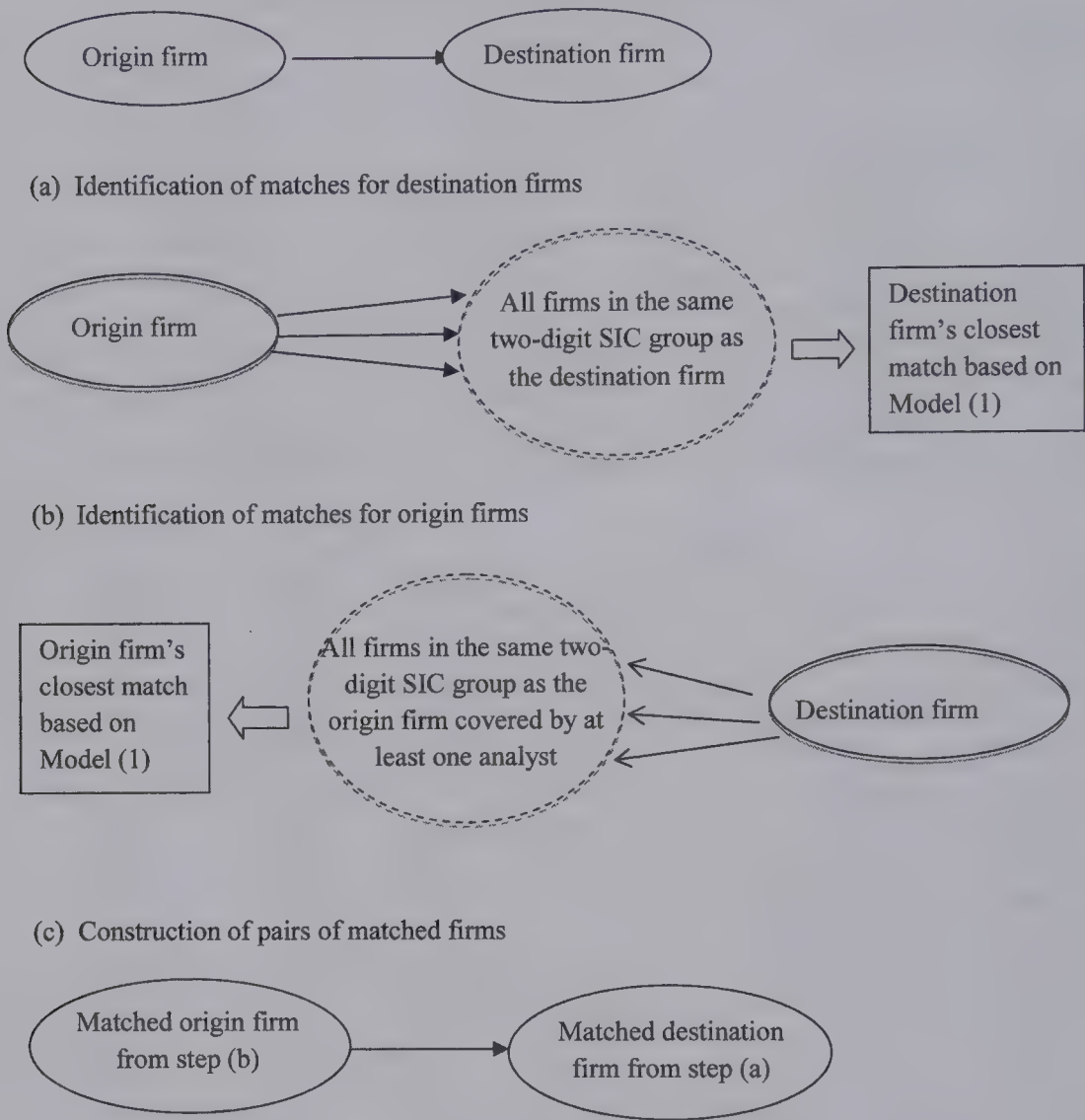
	Origin Firms		Destination Firms		p-value for Differences	
	Mean	Median	Mean	Median	Mean	Median
Full sample (n = 829)						
Analyst Following	17.57	15.00	12.78	10.00	< 0.01	< 0.01
Total Assets (\$ billion)	28.44	2.50	8.80	0.98	< 0.01	< 0.01
Stock Return (%)	3.29	−3.41	2.59	−7.54	0.84	0.03
CEO to CEO moves (n = 94)						
Analyst Following	15.00	13.00	15.04	11.00	0.98	0.20
Total Assets (\$ billion)	12.57	1.31	19.95	1.28	0.53	1.00
Stock Return (%)	−4.71	1.39	−7.94	−10.09	0.63	0.04
Other CEO turnovers (n = 219)						
Analyst Following	22.77	20.00	11.75	9.00	< 0.01	< 0.01
Total Assets (\$ billion)	52.81	7.92	4.59	0.66	< 0.01	< 0.01
Stock Return (%)	−0.36	−4.49	−7.09	−13.07	0.15	0.03
CFO to CFO moves (n = 325)						
Analyst Following	13.16	11.00	12.72	10.00	0.57	0.35
Total Assets (\$ billion)	9.60	1.17	8.80	0.98	0.73	0.18
Stock Return (%)	3.28	−9.51	8.04	−7.28	0.47	0.70
Other CFO turnovers (n = 197)						
Analyst Following	20.78	19.00	12.92	10.00	< 0.01	< 0.01
Total Assets (\$ billion)	40.00	7.86	8.85	1.15	< 0.01	< 0.01
Stock Return (%)	10.68	−1.45	7.77	−4.01	0.67	0.17

Differences shown in bold are significant at the 0.10 level.

This table reports sample selection and descriptive statistics for the executive migration sample. Panel A reports the impact of key sample selection criteria on the number of observations that constitute the final sample. Individual observations are pairs of “origin” and “destination” firms, and executives are said to migrate from origin to destination firms. An observation is classified as CEO (CFO) turnover if an executive joins or departs from an ExecuComp firm as CEO (CFO).

Panel B presents mean/median firm characteristics for origin and destination firms for the full sample and by turnover type. The sample period is from 1994 to 2007.

FIGURE 1
Illustration of Matching Procedure



This figure illustrates the procedure we use to match the turnover sample firms with similar firms unrelated to each other by an executive turnover. For all of our firm-level tests, one observation consists of two firms. An observation is said to be from the turnover sample if an executive goes from one (the origin) to the other (the destination).

Next, we use the estimated coefficients in the two steps above to find (a) a matched firm with closest propensity score to the destination firm and (b) a matched firm with the closest propensity score to the origin firm, step (c) in Figure 1. This matched pair of origin-destination firms is the control sample.

Table 2 presents summary results for the propensity-score-matching procedure. Panel A reports the coefficient estimates and corresponding z-statistics from Model (1) in the two samples. The coefficients are generally consistent with executives moving between firms that are economically more comparable, as reflected in the same four-digit SIC group and higher analyst coverage overlap, and that are relatively larger but underperform. Panel B reports univariate comparisons of turnover and matched observations along the firm-level attributes we use in our analysis of

TABLE 2

Matching Procedure and Descriptive Statistics

Panel A: Logistic Regression to Calculate Propensity Scores

	Destination Firm Choice Holding Origin Firm Constant		Origin Firm Choice Holding Destination Firm Constant	
	Coefficients	Z-Stats	Coefficients	Z-Stats
Recent Turnover	−13.19	−0.21	−6.17***	−4.24
Firm Size Origin	0.04*	1.75	0.65***	17.97
Firm Size Destination	0.47***	17.82	0.01	0.00
Book To Market Origin	0.03	0.53	0.03	0.63
Book To Market Destination	0.00	0.12	0.00	0.23
Stock Return Origin	−0.04	−0.71	−0.12*	−1.82
Stock Return Destination	−0.11**	−2.28	−0.01	−0.30
Analyst Following Origin	−0.01**	−2.98	−0.01	−1.50
Analyst Following Destination	0.01*	1.84	−0.01**	−2.56
Same Four-Digit SIC	0.82***	6.51	0.88***	6.66
Analyst Overlap	0.05***	5.89	0.08***	7.30
n/Pseudo-R ²	219,591	18.23%	93,670	24.13%
Fixed effects	Industry and Year		Industry and Year	

Panel B: Firm Characteristics for Turnover Observations Compared to Matched Observations

Variable	Turnover Sample (n = 829)		Matched Sample (n = 829)		p-value for Differences	
	Mean	Median	Mean	Median	Mean	Median
Same Industry	27.38	0.00	27.38	0.00	NA	NA
Analysts Overlap	1.87	0.00	1.11	0.00	< 0.01	< 0.01
Analyst Following Origin	17.57	15.00	17.49	14.00	0.90	0.62
Analyst Following Destination	12.78	10.00	12.82	9.00	0.94	0.47
Migrate Funds	0.66	1.00	0.29	0.00	< 0.01	< 0.01
Firm Size Origin	8.05	7.83	7.86	7.83	0.05	0.96
Firm Size Destination	7.04	6.89	7.08	7.08	0.62	0.11
Firm Size Difference	−1.02	−0.78	−0.77	−0.73	0.03	0.73
Book To Market Origin	0.47	0.40	0.45	0.37	0.43	0.35
Book To Market Destination	0.43	0.41	0.14	0.43	0.34	0.22
Book To Market Difference	−0.04	0.02	−0.31	0.04	0.37	0.52
Stock Return Origin	3.29	−3.41	5.00	−2.64	0.55	0.52
Stock Return Destination	2.59	−7.54	1.20	−5.38	0.69	0.22
Stock Return Difference	−0.70	−4.71	−3.81	−7.24	0.46	0.30

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively.

Differences shown in bold are significant at the 0.10 level.

Origin (Destination) indicates that the variable is measured in the origin (destination) firm.

Difference indicates that the variable is measured as the difference between the destination and the origin firm.

Panel A reports logistic regression results for the estimation of the conditional probability that an executive moves between two firms. The dependent variable is an indicator equal to 1 if an executive switches between the two firms forming a pair, and 0 otherwise. The independent variables are defined in Appendix A. The first sample consists of the origin firms from which executives are departing paired with all firms in the same two-digit SIC group as the destination firms they are joining. The second sample consists of the destination firms that the executives are joining paired with all firms in the same two-digit SIC group as the origin firm covered by at least one analyst in I/B/E/S. Columns (1) and (2) report coefficient estimates for the first and second samples, respectively.

Panel B reports mean and median values for the control variables in our sample. The sample consists of 829 pairs of companies where a top executive migrates from the origin to the destination company, and 829 pairs of companies with no executive turnover matched on propensity score. The sample period is 1994–2007.

executive-analyst co-migration. The matched observations are insignificantly different from the turnover observations along most, but not all, of the dimensions. Hence, while the multivariate-matching procedure yields a sample that is close to the turnover sample along firm characteristics of the origin and destination firms, we complement the univariate comparison of analyst migration between the two samples with a regression analysis.¹⁴

V. INCIDENCE AND DETERMINANTS OF ANALYST MIGRATION

Incidence of Analyst Migration

We first test H1 by comparing analyst migration at the univariate level in the turnover and the matched samples. We use two variables to assess the degree to which analysts move between origin and destination firms. *Migrate* is an indicator variable equal to 1 if at least one analyst migrates from the origin to the destination firm, 0 otherwise. An analyst is said to migrate (1) if (s)he issues an EPS forecast in the origin firm more than one year prior to the end of the last fiscal year prior to the departure of the migrating executive, and (2) if (s)he issues an EPS forecast for the first time in the destination firm by the end of the first full fiscal year after the migrating executive joins the firm. *Migrants* is the number of analysts migrating from the origin to the destination firm.

Table 3, Panel A presents the results for the full sample. At least one analyst from the origin firm initiates coverage of the destination firm following an executive move in 10.13 percent of our sample observations. This is significantly greater than the 2.90 percent migration in the matched sample. The mean number of migrants (13.87 percent) indicates that for every executive who switches jobs, 0.1387 analysts follow them. This is only slightly higher than the incidence of migration in the turnover sample, indicating usually only one analyst co-migrates. However, it is significantly greater than the mean number of analysts migrating in the matched group (4.34 percent).

Table 3, Panel B presents results for the subset of executives who move within an industry. Analyst migration remains three times as likely in the turnover sample (22.90 percent) compared to the matched sample (7.49 percent). Similar to the full sample, the average analyst migration involves approximately 1.37 analysts. A separate analysis of CEOs and CFOs indicates that within-industry CEO (CFO) turnover is associated with analyst migration in 29.03 percent (18.52 percent) of the sample observations, significantly higher than in the matched samples.

Table 3, Panel C presents results for pairs of origin and destination firms that do not belong to the same industry. When CEO and CFO turnovers are pooled, the incidence of migration is significantly higher in the turnover sample (5.32 percent) than in the matched sample (1.16 percent). In the CFO (CEO) sample, the difference between turnover and matched observations is not statistically significant.

Determinants of Analyst Co-Migration: Firm Characteristics

Research Design

We use the following firm-level logistic regression to investigate the determinants of, and structural constraints to, analyst migration by pooling the turnover and matched samples:

¹⁴ Throughout the paper, we imply that executive migration causes analysts to initiate coverage of the destination firm because of relationships initiated in the origin firm. It is plausible, however, that executive and analyst migration are endogenously determined. To address this concern, we alter our matching procedure by replacing the matched destination firms with the actual destination firms. This allows us to assure it is not unobserved characteristics of the destination firms attracting analysts. We also perform our analysis by replacing the matched origin firms with the actual origin firms. Untabulated results indicate that executive-analyst co-migration is significantly higher for our turnover firms than in either altered matched sample. While we cannot entirely rule out endogeneity of executive and analyst migration, the aforementioned results increase our confidence in attributing our main findings to executive-analyst relationships.

TABLE 3

Analyst Co-Migration–Univariate Analysis

Panel A: Full Sample

Type of Turnover		Turnover Sample	Matched Sample	t-stat for Difference
All (n = 829)	Migrate	10.13	2.90	6.03***
	Migrants	13.87	4.34	4.71***
CEOs (n = 313)	Migrate	11.18	4.47	3.14***
	Migrants	16.61	6.71	2.41**
CFOs (n = 522)	Migrate	9.39	2.06	5.20***
	Migrants	12.07	3.00	4.42***

Panel B: Within Industry

Type of Turnover		Turnover Sample	Matched Sample	t-stat for Difference
All (n = 227)	Migrate	22.90	7.49	4.67***
	Migrants	31.28	11.45	3.31***
CEOs (n = 93)	Migrate	29.03	10.75	3.19***
	Migrants	45.16	16.13	2.36**
CFOs (n = 135)	Migrate	18.52	5.07	3.59***
	Migrants	21.48	7.97	2.64***

Panel C: Across Industries

Type of Turnover		Turnover Sample	Matched Sample	t-stat for Difference
All (n = 602)	Migrate	5.32	1.16	4.09***
	Migrants	7.31	1.66	3.65***
CEOs (n = 220)	Migrate	3.64	1.82	1.17
	Migrants	4.55	2.73	0.83
CFOs (n = 387)	Migrate	6.20	1.01	3.88***
	Migrants	8.79	1.27	3.62***

, * Indicate significance at the 0.05 and 0.01 two-tailed levels, respectively.

This table reports univariate results for the analysis of sell-side analyst migration between pairs of origin and destination firms. The sample consists of 829 pairs of companies where a top executive migrates from the origin to the destination company, and 829 pairs of companies with no executive turnover matched on propensity score. The sample period is 1994–2007. *Migrate* indicates observations where at least one analyst migrates from the origin to the destination firm. *Migrants* is the number of analysts migrating from the origin to the destination firm. All reported means are multiplied by 100.

$$\begin{aligned} \text{Migrate} = & \alpha + \alpha_1 \text{Turnover} + \beta_1 \text{Same Industry} + \beta_2 \text{Analyst Overlap} \\ & + \beta_3 \text{Analyst Following Destination} + \beta_4 \text{Migrate Funds} \\ & + \beta_5 \text{Analyst Following Destination} + \beta_6 \text{Firm Size Difference} \\ & + \beta_7 \text{Book To Market Difference} + \beta_8 \text{Stock Return Difference} + \varepsilon. \end{aligned}$$

(2)

We also run a stacked regression where *Turnover* is interacted with all variables.¹⁵ All variables are defined in Appendix A.

Based on our prediction that relationships between managers and analysts lead to their co-movement across firms, we expect a significantly positive coefficient on *Turnover* (H1). As discussed in Section II, we posit that industry is an important constraint on analysts’ ability to follow managers across firms. Accordingly, we expect a positive coefficient on *Same Industry* and *Analyst Overlap* (H2a(i)). In terms of perceived benefits associated with co-migration, we expect a negative coefficient on the number of analysts following the destination firm (H2a(ii)), and a positive one on *Migrate Funds* (H2a(iii)). Additionally, we control for the number of analysts following the origin firm, which we expect to be positively associated with the likelihood of analyst migration. Finally, we include measures of size, stock performance, and book-to-market ratio, which are common determinants of analyst coverage decisions (Bhushan 1989). We compute for each pair of firms the difference between the variable measured in the destination and the origin firm. For example, *Firm Size Difference* is the logarithm of the destination firm’s total assets minus the logarithm of the origin firm’s total assets.¹⁶

Regression Results

Table 4 reports results for the regression analysis of analyst migration (*Migrate*). The coefficients and marginal effects in the first two columns are estimated based on the full sample of turnover and matched observations. The significantly positive coefficient on *Turnover* ($p < 0.01$) is consistent with the earlier univariate results and with H1. The significantly positive coefficients on *Same Industry* and *Analyst Overlap* show that analysts are more likely to migrate between firms within the same industry, a result consistent with the specialization of analysts by industry (H2a(i)). In contrast, analyst migration exhibits an insignificant association with the number of analysts following the destination firm, which fails to support H2a(ii). Analyst migration is positively associated with mutual fund migration (*Migrate Funds*), suggesting that analysts’ decisions to initiate coverage of destination firms coincides with investor demand for more information about those firms (H2a(iii)). As expected, analyst migration exhibits a positive association with *Analyst Following Origin*, *Firm Size Difference*, and *Stock Return Difference*, although the statistical significance is slightly less than marginal for stock returns ($p = 0.11$). This suggests that analysts initiate coverage of relatively larger, better known, and better performing firms. Finally, the coefficient on *Book To Market Difference* is not significant.

In terms of economic significance, the marginal effect of executive turnover (*Turnover*) on analyst migration is 3.76 percent, which is close to the effect of industry (4.52 percent). Given that the unconditional likelihood of analyst migration in the full sample is 6.51 percent, the relative effect of executive turnover is 58 percent, once accounting for structural constraints. Hence, while the incidence of analyst co-migration is modest, the conditional effect of executive turnover on analysts’ moves is significant.

In the second regression in Table 4, the sample is limited to the turnover observations. The signs of coefficients are generally consistent with the pooled regression. In the third regression,

¹⁵ We also examine a specification using a conditional logistic model. That sample includes only pairs of turnovers and matches for which the response variable differs between the treatment and control observations, drastically reducing the sample and excluding the *Same Industry* indicator. The coefficient on *Turnover* remains significant (not tabulated).

¹⁶ We also add measures of investment banking activity and annual trading volume in origin and destination firms as additional control variables in our tests of Model (2), because they are sources of revenue streams for analysts’ employers. We find that those variables exhibit no significant association with analyst migration and do not affect the inferences on the turnover variable.

TABLE 4
Determinants of Co-Migration
Firm Characteristics

	Hypothesis: Predicted Sign	Turnover and Matched Sample		Turnover Sample (1)		Matched Sample (2)		(1) – (2) Chi-Square (p-values)
		Coefficients (p-values)	Marginal Effects	Coefficients (p-values)	Marginal Effects	Coefficients (p-values)	Marginal Effects	
Intercept		–5.06*** (< 0.01)		–3.84*** (< 0.01)		–5.30*** (< 0.01)		7.11*** (< 0.01)
Turnover	H1: +	1.13*** (< 0.01)	3.76					
Same Industry	H2a(i): +	1.36*** (< 0.01)	4.52	1.19*** (< 0.01)	7.05	1.82*** (< 0.01)	3.02	1.11 (0.29)
Analysts Overlap	H2a(i): +	0.03*** (< 0.01)	0.33	0.15*** (< 0.01)	0.88	0.04 (0.15)	0.06	6.28*** (0.01)
Analyst Following Destination	H2a(ii): –	–0.01 (0.34)	–0.03	–0.03** (0.04)	–0.18	0.01 (0.34)	0.02	4.25*** (0.04)
Migrate Funds	H2a(iii): +	0.62*** (0.02)	2.05	0.47 (0.11)	2.79	0.82* (0.05)	1.37	0.44 (0.51)
Analyst Following Origin	+	0.04*** (< 0.01)	0.11	0.05*** (< 0.01)	0.27	0.02 (0.14)	0.03	1.77 (0.18)
Firm Size Difference	+	0.12* (0.08)	0.41	0.18* (0.06)	1.07	0.07 (0.57)	0.11	0.51 (0.48)
Book To Market Difference	–	0.01 (0.72)	0.03	0.00 (0.97)	0.01	0.04 (0.66)	0.07	0.17 (0.68)
Stock Return Difference	+	0.07 (0.51)	0.23	0.13 (0.26)	0.78	–0.05 (0.83)	–0.09	0.47 (0.49)
n		1,658		829		829		
Pseudo R ²		22.35%		22.36%		17.77%		

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively. Reported marginal effects are multiplied by 100. The “Hypothesis: Predicted Sign” column indicates the predicted sign for the coefficient on each variable, and the corresponding hypothesis when applicable. This table reports logistic regression results for the analysis of sell-side analyst migration between companies. The dependent variable, *Migrate*, is an indicator variable equal to 1 if at least one analyst from the origin firm initiates coverage of the destination firm, 0 otherwise. The sample consists of 829 pairs of origin/destination companies where a top executive from the origin company joins the destination company, and 829 pairs of companies with no executive turnover matched on propensity score. The sample period is 1994–2007.

the sample includes only the matched observations. Comparing coefficients across both samples enables us to see if firm economic attributes affect analyst coverage decisions differently, depending on whether there is an executive move. The fourth column reports Wald Chi-square statistics for the difference between coefficients from Column (2) and Column (3) from a stacked regression. The significant difference between the two intercepts ($p < 0.01$) shows that the significant coefficient on *Turnover* in Column (1) is robust to its interaction with all variables in the analysis. The significantly positive coefficient on *Analyst Overlap* in the turnover sample is also significantly greater than the one in the matched sample, which further highlights the importance of industry, which is not captured by two-digit SIC, in driving analyst co-migration. The negative coefficient on *Analyst Following Destination* in the turnover sample is also significantly different than in the matched sample, which suggests that the interpretation of this coefficient as the result of competition in the destination firm holds only in the context of executive turnover. Hence, we find support for H2a(ii) only in the turnover sample. The remaining differences between the turnover and matched samples are not statistically significant, consistent with those economic factors being structural constraints that drive analysts' coverage decisions, irrespective of relationships.¹⁷

VI. PROPERTIES OF MIGRATING ANALYSTS' COVERAGE AND FORECASTS

Having established that some sell-side analysts do follow executives across firms, we analyze properties of analyst coverage of the origin and destination firms around the migration to determine whether they appear to have a stronger relationship as proxied by (1) covering the stocks more actively, (2) an informational advantage, and/or (3) issuing more favorable recommendations.

We use the following analyst-level logistic regression model to examine analysts' outputs in their coverage of the origin and destination firms:

$$\begin{aligned} \text{Migrant} = & \alpha + \beta_1 \text{Years of Coverage} + \beta_2 \text{Frequency of Forecast Revisions} \\ & + \beta_3 \text{Forecast Horizon} + \beta_4 \text{Meeting with Management} \\ & + \beta_5 \text{Absolute Forecast Error} + \beta_6 \text{Recommendation Profitability} \\ & + \beta_7 \text{Strong Buy Recommendation} + \beta_8 \text{School Tie} + \beta_9 \text{Brokerage Overlap} \\ & + \Sigma \gamma_i \text{Fixed Effects} + \varepsilon. \end{aligned} \tag{3}$$

All variables are defined in Appendix A. The unit of analysis is analyst-firm pairs. The sample consists of either all firms covered by migrating analysts during the period of interest (comparative advantage) or all analysts covering the origin or destination firm (competitive advantage). Accordingly, the dependent variable is a binary variable equal to 1 if the analyst is a migrating analyst and the covered firm is either the origin or the destination firm, depending on the context in which the manager/analyst relationship is assessed, and 0 otherwise.

¹⁷ To further address endogeneity concerns, we partition our sample of executive migration by identifying plausibly exogenous turnovers. We use executive-board social ties as an instrument as they are likely to influence executive's decisions to join destination firms (and/or destination firms' boards to hire certain executives), but not analysts' coverage decisions. Using executives' and directors' educational background from BoardEx, we code executive migrations as connected if the incoming executive went to the same university as any of the destination firm's board members (136 turnovers), and not connected otherwise (693 turnovers). Untabulated results indicate that analyst migration is significantly higher in our turnover firms than in the matched sample, regardless of whether the executive is connected to the destination firm's board. Similarly, we also control for school ties between analysts and board members of the destination firm, but find no indication that those ties influence analyst migration (not tabulated).

Determinants of Co-Migration: Analyst Characteristics

In origin firms, we expect migrating analysts to have more experience, i.e., we expect a positive coefficient on *Years of Coverage*.¹⁸ We also expect migrating analysts to revise their forecasts more frequently and to be more likely to issue long-term forecasts. To capture quality professional interactions we include *Meeting with Management*, an indicator variable equal to 1 if the analyst issues a report following an exclusive meeting with the firm's top management. We expect migrating analysts to be more likely to meet with management. All of the preceding variables proxy for analyst coverage intensity (H2b(i)). We expect migrating analysts to issue more accurate forecasts and more profitable recommendations (H2b(ii)).¹⁹ Finally, if migrating analysts hold more favorable views of the managers they follow, then we expect more positive recommendations (H2b(iii)). We control for differences across firms covered by migrating analysts by subtracting firm level medians for *Absolute Forecast Error*, *Recommendation Profitability*, and *Strong Buy Recommendation* in comparative adjustments tests. This mitigates concerns that origin firms are simply easier to follow than other firms.

We also include an indicator for school ties between analysts and executives, to ensure that our results are incremental to those documented in Cohen et al. (2010). Additionally, we control for incidences where the analyst's employer already covers the destination firm. We expect analysts to be less likely to displace their colleagues by migrating under those circumstances. We include broker and year fixed effects to control for differences across observations that are not captured by the variables of interest.²⁰ We also include the firm-level variables from Model (1) in the competitive advantage tests.

Table 5 shows the results of the analysis of origin firm coverage before executives' departure. The univariate results in Panel A indicate migrating analysts have an average tenure of 3 years in the origin firm, which is significantly greater than the 2.07 years of coverage by non-migrating analysts. They issue more one-year-ahead EPS forecasts per year in origin firms (4.12 versus 3.18 for other firms and 3.60 for non-migrating). The average longest forecast horizon of migrating analysts in origin firms (2.20) is also significantly higher both comparatively and competitively. Analyst reports explicitly referring to meetings with management are also more frequent for migrating analysts in origin firms (7 percent) than other firms and other analysts (2 percent). Collectively, these results suggest that co-migrating analysts cover the origin firm more intensely (H2b(i)). Mean absolute value of one-year-ahead EPS forecast errors scaled by price are smaller for migrating analysts at 1.11 percent for origin firms compared to 1.88 in other firms, consistent with co-migrating analysts being more accurate in their coverage of the origin firm (H2b(ii)). However, inconsistent with H2b(ii), their recommendations appear to be significantly less profitable in origin firms.

¹⁸ We measure *Years of Coverage* regardless of the executive's tenure. We also use an alternative measure of *Years of Coverage* by censoring the variable at the executive's tenure length, if the analyst tenure exceeds it. Our results are qualitatively unchanged.

¹⁹ We also use target price forecast accuracy as a measure of information advantage. However, these data are only available after 2000 and then for a subsample of analysts, reducing our analyses to one third of the sample. Untabulated results show no robust evidence of migrating analysts issuing more accurate target prices.

²⁰ Except for *Meeting with Management*, the variables in this analysis are outputs from which we can only indirectly infer the likelihood that an analyst has a relationship with a manager. Coverage intensity and information advantage are functions of many variables such as analyst experience, talent, effort, etc. In untabulated results, we control for various analyst and broker characteristics, including analyst seniority in I/B/E/S, number of firms and industries covered, and number of analysts employed by the brokerage house. While some of those variables exhibit a significant association with analyst migration, their inclusion does not affect the significance of our results. Also, note that comparative advantage tests implicitly control for analysts' characteristics.

TABLE 5

Determinants of Co-Migration: Analyst Characteristics

	Comparative Advantage			Competitive Advantage		
	Origin Firm	Other Firms	t-stats	Migrating	Non-Migrating	t-stats
	(n = 115)	(n = 1955)		(n = 115)	(n = 1987)	
<i>Years of Coverage</i>	3.00	2.66	1.42	3.00	2.07	3.85***
<i>Frequency of Forecast Revisions</i>	4.12	3.18	4.33***	4.12	3.60	2.35**
<i>Forecast Horizon</i>	2.20	2.13	3.09***	2.20	2.10	1.65*
<i>Meeting with Management</i>	0.07	0.02	1.79*	0.07	0.02	1.97*
<i>Absolute Forecast Error (%)</i>	1.11	1.88	-3.89***	1.11	1.16	-0.35
<i>Recommendation Profitability (%)</i>	-1.16	0.71	-2.08**	-1.16	0.05	-1.76*
<i>Strong Buy Recommendation</i>	0.27	0.21	1.20	0.27	0.23	0.65
<i>School Tie</i>	0.03	0.03	0.15	0.03	0.02	0.29

	Hypothesis: Predicted Sign	Comparative Advantage		Competitive Advantage	
		Coefficients	Coefficients	Coefficients	Coefficients
		(p-values)	(p-values)	(p-values)	(p-values)
<i>Years of Coverage</i>	H2b(i): +	0.84*** (< 0.01)	0.14* (0.09)	0.10** (0.01)	0.12 (0.19)
<i>Frequency of Forecast Revisions</i>	H2b(i): +	0.10** (0.01)	0.15** (0.05)	0.11*** (< 0.01)	0.18** (0.03)
<i>Forecast Horizon</i>	H2b(i): +	0.11*** (< 0.01)	0.12 (0.77)	0.65*** (0.01)	0.35 (0.30)
<i>Meeting with Management</i>	H2b(i): +	0.65*** (0.01)	1.17** (0.04)	1.56*** (< 0.01)	0.86 (0.36)
<i>Absolute Forecast Error (%)</i>	H2b(ii): -	-1.56*** (< 0.01)	-0.71 (0.17)	-0.16*** (< 0.01)	-0.13 (0.31)
<i>Recommendation Profitability</i>	H2b(ii): +		-4.07* (0.08)		-4.84** (0.04)
<i>Strong Buy Recommendation</i>	H2b(iii): +		0.71 (0.13)		0.24 (0.62)
<i>School Tie</i>	+		0.29 (0.75)		-8.37*** (< 0.01)
<i>Brokerage Overlap</i>	-			0.84*** (< 0.01)	1.76*** (< 0.01)
Firm-level controls		—	—	Included	Included
Broker and Year fixed effects		Included	Included	Included	Included
n		1,859	718	1,934	889
Pseudo R ²		5.93%	11.79%	14.37%	20.90%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively. Standard errors are clustered by analyst. The “Hypothesis: Predicted Sign” column indicates the predicted sign for the coefficient on each variable, and the corresponding hypothesis when applicable. This table reports univariate and regression results for the analysis of properties of migrating analysts’ coverage in origin firms compared to (1) their coverage in other firms (comparative advantage) and (2) other analysts covering the origin firms (competitive advantage). The variables are measured over years -2 and -1, where year 0 is the year in which the executive joins the destination firm. Units of observations are analyst-firm pairs. In Panel B, the dependent variable is equal to 1 if the analyst subsequently initiates coverage of the departing executive’s new firm and if the covered firm is the origin firm, and 0 otherwise.

Table 5, Panel B presents logistic regression results with migrating analysts/firm observations coded as 1. The first and third columns report results for the full sample, in terms of comparative and competitive advantage, respectively. In the first column, we find that migrating analysts have covered firms for more years, forecast more frequently, issue longer horizon forecasts, and meet with management more often. Hence, migrating analysts' coverage of the origin firm exhibits greater intensity than their coverage of other firms, consistent with H2b(i). The significantly negative coefficient on *Absolute Forecast Error* indicates that migrating analysts are more accurate in predicting annual EPS in the origin firm compared to other firms they cover (H2b(ii)). However, in the second column, we find no evidence of more profitable or favorable coverage in origin firms.²¹ The negative coefficient on *Recommendation Profitability* (H2b(ii)) suggests that relationships may induce some optimistic bias in the recommendations of co-migrating analysts. Our results are thus consistent with prior research that documents biases affecting analysts' recommendations (Bradshaw 2004; Barniv, Hope, Myring, and Thomas 2009), which analysts do not seem to overcome in the context of relationships. Consistent with the univariate results, the insignificant coefficient on *Strong Buy Recommendation* does not support H2b(iii) either. The results in terms of competitive advantage are qualitatively similar, suggesting that migrating analysts cover origin firms more intensely and accurately but less profitably than do other analysts. Interestingly, brokerage overlap is positively and significantly associated with migration, suggesting that relationships may in fact be strong enough to induce brokerage houses to re-assign analysts covering the destination firm.

Consequences of Co-Migration: Analysts' Output for Origin Firms

We report the results of the analysis of origin firm coverage after executives' departure in Table 6. According to H3a, we expect migrating analysts to no longer exhibit any advantage in covering the origin firms. In Panel A, the univariate results indicate that migrating analysts exhibit no significant comparative or competitive advantage in their coverage of the origin firm post executive departure, except in terms of meetings with management and recommendation profitability (competitively). In Panel B, the regression results include analysts' significantly greater propensity to meet with the new management team, as reflected in the positive coefficient on *Meeting with Management* (H3a(i)) in the first column) and issuance of more favorable recommendations, as reflected in the positive coefficient on *Strong Buy Recommendation* (H3a(iii)) in the second column) for the origin firm than other firms they cover. Otherwise, migrating analysts do not exhibit any significant difference in their coverage of the origin firm. Hence, while the positive coefficients on *Meeting with Management* and *Strong Buy Recommendation* suggests that analysts who do not drop coverage of the origin firm may try to establish a relationship with the incoming manager by meeting with them and issuing more favorable recommendations, the results in Table 6 are broadly consistent with H3a. That is, once the old manager has departed, migrating analysts exhibit no advantage in covering the origin firm.²²

²¹ Note that the coefficients on years of coverage, frequency of forecast revisions, etc., tend to lose some of their significance when we include recommendations and/or school ties, as the results in columns 3 and 4 indicate. However, further investigation indicates that the weaker results are due to the reduced power in the smaller sample rather than collinearity between the variables included in both models and analyst recommendations (not tabulated).

²² We draw our inferences in Table 6 by comparing coefficients to zero (H3a). As an alternative, we could compare their pre- and post-coverage properties. In untabulated results, we find that migrating analysts exhibit a statistically significant reduction in their frequency of forecast revisions, and a marginally significant reduction in forecast accuracy compared to other analysts.

TABLE 6
Consequences of Co-Migration
Analysts' Output for Origin Firms

Panel A: Univariate Analysis

	Comparative Advantage			Competitive Advantage		
	Origin Firm (n = 87)	Other Firms (n = 2058)	t-stats	Migrating (n = 87)	Non-Migrating (n = 1847)	t-stats
<i>Years of Coverage</i>	4.19	2.40	5.70***	4.19	2.34	6.17***
<i>Frequency of Forecast Revisions</i>	3.62	3.48	0.47	3.62	3.69	0.21
<i>Forecast Horizon</i>	2.28	2.26	0.28	2.28	2.28	-0.07
<i>Meeting with Management</i>	0.07	0.03	2.27**	0.07	0.02	2.00**
<i>Absolute Forecast Error (%)</i>	2.50	2.53	-0.04	2.50	2.07	0.53
<i>Recommendation Profitability (%)</i>	2.66	2.15	1.13	2.66	0.47	2.19**
<i>Strong Buy Recommendation</i>	0.31	0.21	1.62	0.31	0.19	1.59
<i>School Tie</i>	0.03	0.03	0.29	0.02	0.02	0.19

Panel B: Regression Analysis

	Hypothesis: Predicted Sign	Comparative Advantage		Competitive Advantage	
		Coefficients (p-values)	Coefficients (p-values)	Coefficients (p-values)	Coefficients (p-values)
<i>Years of Coverage</i>	H3a(i): +	0.29*** (< 0.01)	0.45*** (< 0.01)	0.20*** (< 0.01)	1.93* (0.05)
<i>Frequency of Forecast Revisions</i>	H3a(i): +	-0.04 (0.54)	-0.21 (0.14)	-0.10 (0.31)	0.05 (0.80)
<i>Forecast Horizon</i>	H3a(i): +	0.62 (0.11)	1.61** (0.02)	0.55 (0.15)	2.43 (0.21)
<i>Meeting with Management</i>	H3a(i): +	1.96*** (< 0.01)	2.49 (0.12)	1.69 (0.12)	2.20 (0.29)
<i>Absolute Forecast Error (%)</i>	H3a(ii): -	-0.48 (0.34)	0.78 (0.66)	0.01 (0.34)	-0.15 (0.25)
<i>Recommendation Profitability</i>	H3a(ii): +		-3.88** (0.04)		9.47 (0.38)
<i>Strong Buy Recommendation</i>	H3a(iii): +		2.58*** (< 0.01)		3.47 (0.26)
<i>School Tie</i>	+		-0.21 (0.91)		-6.23*** (< 0.01)
<i>Brokerage Overlap</i>	-			0.77 (0.13)	1.76*** (< 0.01)
Firm-level controls		—	—	Included	Included
Broker and Year fixed effects		Included	Included	Included	Included
n		1,972	648	1,620	559
Pseudo R ²		13.82%	30.18%	15.26%	56.62%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively. Standard errors are clustered by analyst. The "Hypothesis: Predicted Sign" column indicates the predicted sign for the coefficient on each variable, and the corresponding hypothesis when applicable. This table reports univariate and regression results for the analysis of properties of migrating analysts' coverage in origin firms compared to (1) their coverage in other firms (comparative advantage) and (2) other analysts covering the origin firms (competitive advantage). The variables are measured over years 0 and +1, where year 0 is the year in which the executive joins the destination firm. Units of observations are analyst-firm pairs. In Panel B, the dependent variable is equal to 1 if the analyst initiates coverage of the departing executive's new firm and if the covered firm is the origin firm, and 0 otherwise.

Consequences of Co-Migration: Analysts' Output for Destination Firms

Finally, we look at properties of migrating analysts' coverage in destination firms, to test whether the relationship they developed with the migrating executive transfers from the origin to the destination firm in terms of coverage intensity and information advantage (H3b).

The univariate results in Table 7, Panel A fail to indicate any statistically significant advantage for migrating analysts, either comparatively or competitively, except that they report meeting with management more frequently (0.06) than other analysts (0.02).

In Table 7, Panel B the regression results indicate that migrating analysts exhibit no significant comparative advantage in their coverage of the destination firm (first two columns). They do, however, issue significantly more positive recommendations for the destination firm, which is consistent with a revealed more positive belief in the new management team they decided to follow (H3b(iii)).

In terms of competitive advantage, in the third column, we find weak evidence of some advantage. The migrating analysts meet with management more often, which gives limited support to H3b(i). In the full sample they also are more accurate than their peers in predicting the destination firm's EPS, consistent with H3b(ii), but this result is no longer significant when restricted to the sample of firms for which we have recommendations. Overall, we find limited evidence that the executive-analyst relationships carry over in terms of one-on-one meetings and forecast accuracy, but the significance remains limited.^{23,24}

VII. ADDITIONAL ANALYSES

Migrating Analysts' Incentives

We have focused primarily on informational advantages that are easily identifiable and that proxy for direct advantages that exist for the analysts. However, providing an informational insight is only one portion of analysts' jobs. Analysts are also a crucial conduit for providing investors access to managers. In fact, over one third of institutional commissions are allocated based on the analysts' ability to provide such access (Bushee et al. 2011). Our discussions with analysts indicate that they value this ability, which is built up over time, and that it is one of the primary benefits of maintaining relationships with managers who change firms. That is, the analysts may be able to increase the number of firms for which they can provide access in a relatively quick and low-cost manner in comparison to initiating coverage of a previously unknown firm. While we cannot directly observe the benefit to analysts, we can examine whether they increase their overall market capitalization covered, that is, does the new firm increase the analysts' portfolio or just substitute for another previously covered firm. We compare the change in portfolio size for migrating analysts to

²³ We also restrict our competitive advantage analysis to the sample of analysts who initiate coverage of the destination firm around the same time as the migrating analysts. This creates a sample in which all analysts face the same learning curve. However, there are only 136 observations that meet these criteria resulting in an *ex ante* low-power test. The results (not tabulated) are similar to those documented in the full sample. That is, they indicate more meetings with management, but no other items are statistically different. We further examine the impact of a learning curve by extending the post-migration time period to three years. In untabulated results, we find no evidence of improvement over time.

²⁴ We also partition the sample based on whether destination firms issue earnings forecasts, which could affect the information advantage of migrating analysts. We find some untabulated evidence that migrating analysts are more accurate in destination firms that do *not* issue earnings guidance, which suggests that knowing the manager gives them some advantage when others cannot rely on firm-provided forecasts. Last, we distinguish between migrating analysts who continue to cover the origin firm and those who cease to (15 percent). The latter exhibit more accurate forecasts in the destination firm, which suggests that dropping coverage of the origin firm is a revealed preference indicative of a stronger relationship (not tabulated).

TABLE 7
Consequences of Co-Migration
Analysts' Output for Destination Firms

Panel A: Univariate Analysis

	Comparative Advantage			Competitive Advantage		
	Destination Firm (n = 115)	Other Firms (n = 2034)	t-stats	Migrating (n = 115)	Non-Migrating (n = 1848)	t-stats
<i>Years of Coverage</i>	0.70	2.57	−8.57***	0.70	1.60	−4.76***
<i>Frequency of Forecast Revisions</i>	3.17	3.51	−1.37	3.17	3.29	−0.59
<i>Forecast Horizon</i>	2.21	2.26	−0.21	2.21	2.23	−0.38
<i>Meeting with Management</i>	0.06	0.03	1.64	0.06	0.02	1.82*
<i>Absolute Forecast Error (%)</i>	1.96	2.56	−0.61	1.96	3.73	−1.40
<i>Recommendation Profitability (%)</i>	4.22	2.13	0.65	4.22	1.23	0.95
<i>Strong Buy Recommendation</i>	0.25	0.21	1.25	0.25	0.22	0.71
<i>School Tie</i>	0.03	0.03	0.11	NA	NA	NA

Panel B: Regression Analysis

	Hypothesis: Predicted Sign	Comparative Advantage		Competitive Advantage	
		Coefficients (p-values)	Coefficients (p-values)	Coefficients (p-values)	Coefficients (p-values)
<i>Years of Coverage</i>		−0.43*** (< 0.01)	−0.45*** (< 0.01)	−0.28** (0.01)	−0.37*** (0.01)
<i>Frequency of Forecast Revisions</i>	H3b(i): +	−0.04 (0.51)	−0.05 (0.47)	−0.01 (0.87)	−0.06 (0.36)
<i>Forecast Horizon</i>	H3b(i): +	0.10 (0.71)	0.08 (0.80)	0.05 (0.82)	−0.05 (0.86)
<i>Meeting with Management</i>	H3b(i): +	0.47 (0.42)	0.29 (0.64)	1.28*** (0.01)	2.26*** (< 0.01)
<i>Absolute Forecast Error (%)</i>	H3b(ii): −	0.00 (0.98)	0.00 (0.99)	−0.02** (0.05)	−0.05 (0.22)
<i>Recommendation Profitability</i>	H3b(ii): +		0.71 (0.17)		0.58 (0.42)
<i>Strong Buy Recommendation</i>	H3b(iii): +		0.68** (0.02)		0.05 (0.89)
Firm-level controls		—	—	Included	Included
Broker and Year fixed effects		Included	Included	Included	Included
n		1,972	1,011	1,780	1,150
Pseudo R ²		9.85%	13.12%	12.76%	15.03%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively. Standard errors are clustered by analyst. The “Hypothesis: Predicted Sign” column indicates the predicted sign for the coefficient on each variable, and the corresponding hypothesis when applicable. This table reports univariate and regression results for the analysis of properties of migrating analysts’ coverage in destination firms compared to (1) their coverage in other firms (comparative advantage) and (2) other analysts covering the origin firms (competitive advantage). The variables are measured over years 0 and +1, where year 0 is the year in which the executive joins the destination firm. Units of observations are analyst-firm pairs. In Panel B, the dependent variable is equal to 1 if the analyst initiates coverage of the departing executive’s new firm and if the covered firm is the origin firm, and 0 otherwise.

that of their peers within the same employer with the closest portfolio size, and among other analysts who cover the origin firms from brokerage firms that employ a similar number of analysts. Portfolio size is measured as the logged aggregate market capitalization of all firms covered by an analyst.

Table 8, Panel A shows that migrating analysts experience a significantly greater increase in the aggregate market capitalization of their portfolio compared to their peers. The coefficient on *Migrating Analyst* is positive and statistically significant whether the benchmark is in-house peers as shown in the first column or similar-employer peers as shown in the second column. This suggests that migrating analysts may benefit from following managers and provide institutions access to managers for a set of firms with greater market capitalization.

The benefits of executive-analyst co-migration need not only accrue to analysts. While we do not consider executives' incentives in our setting, we briefly examine the consequences of the changes for destination firms. Insofar as greater analyst coverage brings about more visibility and liquidity to firms (Healy, Hutton, and Palepu 1999), destination firms may benefit from the added coverage by migrating analysts. Table 8, Panel B reports regression results where the dependent variable is change in analyst coverage scaled by firm size (Δ *Analyst Following*), and compares coefficients between the turnover sample and matched sample from the analyses in Tables 3 and 4. In the turnover sample, the significantly positive coefficient on *Number of Analysts Migrating* indicates that firms that hire managers who co-migrate with analysts experience a significantly greater change in coverage than those that hire executives who do not co-migrate with analysts. In contrast, the coefficient is not significant in the matched sample, and the coefficient is significantly higher in the turnover than in the matched sample. That is, the effect of executive-analyst co-migration on destination firms' coverage is significantly greater than that of analyst migration driven by other factors. Hence, the potential benefits of migratory relationships need not be limited to the individual executives and analysts.

Regulation Fair Disclosure

Prior research shows that Reg FD affected the interaction between firm managers and sell-side analysts (Francis, Nanda, and Wang 2006; Wang 2007; Cohen et al. 2010). In particular, Cohen et al. (2010) show that Reg FD reduced the value of some relationships. However, many of the benefits of a strong relationship discussed in the previous sections are either not directly related to information, for example, professional and personal favors, or can increase the analysts' ability to effectively understand and forecast the firm, for example, from a deeper understanding of managerial operating or communication style. These benefits are less likely to have been impacted by Reg FD. In fact, the latter benefits may have become more important post-Reg FD as analysts must build their own expectations of performance rather than relying on inside information from managers. To verify that our main results hold throughout different institutional regimes, we re-run the analysis by splitting the sample between turnovers that occurred from 1994 to 2000 included (pre-Reg FD) and from 2001 to 2007 (post-Reg FD).

In Table 9, Panel A the firm-level results indicate that executive-analyst co-migration is significantly greater than in the matched sample post-Reg FD, although not for CEO migrations. In Table 9, Panel B the analyst-level results are qualitatively similar to those reported for the full sample in Table 5, indicating that migrating analysts tend to exhibit more intense coverage of origin firms before the executives' departures. As in the full sample, the analysts' advantage does not transfer to the destination firms, although the evidence suggests that they are more likely to meet with the new manager. Untabulated regression results also confirm that our main results hold after the passage of Reg FD. Overall, the post-Reg FD evidence supports our hypothesis that the relationships we observe are not driven by selective disclosure between executives and analysts.

TABLE 8
Consequences of Co-Migration
Analysts' Incentives and Destination Firm Coverage

Panel A: Change in Analyst Portfolio Size for Migrating Analysts

	Predicted Sign	Matched with In-House Peer		Matched with Peers in Competing Firms	
		Coefficient	(p-value)	Coefficient	(p-value)
Intercept		0.51***	(< 0.01)	0.48***	(< 0.01)
Migrating Analyst	+	0.32**	(0.02)	0.26***	(< 0.01)
S&P500 Return	+	-0.46	(0.19)	0.01	(0.97)
Seniority	?	-0.03**	(0.03)	-0.01	(0.26)
Beginning Portfolio Size	-	-0.09***	(< 0.01)	-0.15***	(< 0.01)
Fixed effects		Broker		Broker	
n		204		324	
Adj. R ²		9.00%		7.51%	

Panel B: Change in Analyst Coverage for the Destination Firm

	Predicted Sign	Turnover Sample		Matched Sample		t-stat for Difference
		Coefficient	(p-value)	Coefficient	(p-value)	
Intercept		0.08	(0.48)	0.06	(0.49)	0.13
Number of Analysts Migrating	+	0.24***	(< 0.01)	0.03	(0.69)	1.91*
Analyst Following	-	-0.03***	(< 0.01)	-0.01***	(< 0.01)	-3.70***
Firm Size	+	0.01	(0.66)	0.01	(0.28)	-0.32
Mutual Fund Migration	+	0.28***	(< 0.01)	0.09*	(0.06)	2.59***
Stock Return	+	0.19***	(< 0.01)	0.25***	(< 0.01)	-0.71
Book to Market	-	0.00	(0.94)	0.01***	(< 0.01)	-0.11
n		829		829		
Adj. R ²		19.01%		9.81%		

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively.

This table reports regression results for the analysis of changes in analysts' portfolio size and firms' analyst coverage around executive and analyst migration.

In Panel A, the dependent variable, $\Delta Portfolio\ Size$, is the change in the natural logarithm of the total market capitalization of firms covered by analysts from year -1 to year +1, where year 0 is the year in which an executive joins the destination firm. The sample consists of migrating analysts, their two closest peers among other analysts covering the origin firm (in terms of brokerage firm size), and their closest peer among other analysts working for the same brokerage firm (in terms of portfolio size as of year -1).

In Panel B, the dependent variable, $\Delta Analyst\ Following$, is the net change in analyst coverage of destination firms from year -1 to year +1, where year 0 is the year in which an executive joins the destination firm, scaled by the firm's market capitalization. The sample consists of all destination firms joined by executives in our sample and their matches.

Variable Definitions:

Migrating Analyst = analyst migrated from the origin to the destination firm;

S&P500 Return = buy-and-hold return on the S&P 500 Index from year -1 to year +1 included;

Seniority = number of years the analyst has been employed by the same brokerage house, based on his/her first forecast issued in I/B/E/S with the same broker/analyst code;

Beginning Portfolio Size = natural logarithm of the total market capitalization of firms covered by analysts as of the end of year -1;

Number of Analysts Migrating = number of analysts covering the incoming executive's origin firm (or its matched firm) who initiate coverage of the destination firm;

Mutual Fund Migration = firms in which mutual funds that previously invested in the origin firm (or its matched firm) take a position; and

Analyst Following, Firm Size, Book to Market, and Stock Return = as defined in Appendix A.

TABLE 9
Summary Results for Post-Reg FD Period

Panel A: Univariate Analysis—Analyst Migration

		Turnover Sample	Matched Sample	t-stat for Difference
All (n = 479)	Migrate	10.86	3.55	4.41***
	Migrants	13.57	5.64	3.24***
CEOs (n = 153)	Migrate	11.11	6.54	1.41
	Migrants	13.07	9.80	0.70
CFOs (n = 326)	Migrate	10.74	2.15	4.53***
	Migrants	13.80	3.68	3.55***

Reported means are multiplied by 100.

Panel B: Univariate Analysis—Determinants of Co-Migration, Analyst Characteristics

	Comparative Advantage			Competitive Advantage		
	Origin Firm (n = 65)	Other Firms (n = 1019)	t-stats	Migrating Analyst (n = 65)	Other Analysts (n = 1180)	t-stats
Years of Coverage	2.57	2.48	0.32	2.57	1.80	2.73***
Frequency of Forecast Revisions	4.25	3.42	2.70***	4.25	3.58	2.20**
Forecast Horizon	2.42	2.25	2.36**	2.42	2.22	2.71***
Meeting with Management	0.09	0.04	1.59	0.09	0.03	1.69*
Absolute Forecast Error (%)	1.06	1.71	−2.64***	1.06	1.00	0.23
Recommendation Profitability (%)	−0.60	0.85	−1.24	−0.60	0.49	−1.03
Strong Buy Recommendation	0.21	0.18	0.55	0.21	0.24	−0.42

Panel C: Univariate Analysis—Migrating Analysts’ Coverage of Destination Firms

	Comparative Advantage			Competitive Advantage		
	Destination Firm (n = 65)	Other Firms (n = 1157)	t-stats	Migrating Analyst (n = 65)	Other Analysts (n = 1227)	t-stats
Years of Coverage	0.75	2.41	−8.03***	0.75	1.66	−3.30***
Frequency of Forecast Revisions	3.17	3.80	−2.22**	3.17	3.44	−0.89
Forecast Horizon	2.26	2.37	−0.49	2.26	2.32	−0.84
Meeting with Management	0.11	0.04	1.80*	0.11	0.03	2.06**
Absolute Forecast Error (%)	1.48	2.50	−1.09	1.48	1.21	0.52
Recommendation Profitability (%)	1.23	1.47	−0.12	1.23	1.29	0.03
Strong Buy Recommendation	0.15	0.15	0.18	0.15	0.16	−0.20

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 two-tailed levels, respectively. This table reports univariate results for the post-Reg FD period of our sample. Panel A presents univariate results for the analysis of sell-side analyst migration between pairs of origin and destination firms. The sample consists of 479 pairs of companies where a top executive migrates from the origin to the destination company, and 479 pairs of companies with no executive turnover matched on propensity score. Panels B and C report univariate results on properties of migrating analysts’ coverage of origin and destination firms around executives’ departures compared either to their own coverage and forecasts in other firms (comparative advantage) or to other analysts covering the origin firms (competitive advantage). The sample is limited to origin/destination firms where at least one analyst migrates. Panel B (C) reports those results for analysts’ coverage of origin (destination) firms before (after) executives’ departure.

VIII. CONCLUSION

This paper examines whether manager/sell-side analyst relationships developed through repeated business interactions provide incentives for analysts to migrate to a new firm when managers change employment. We argue that capital market participants are likely to undertake such migrations in order to maintain valuable relationships with the managers. However, structural constraints are likely to limit such analyst movement. We find analysts are likely to initiate coverage of the new firm, but to a limited extent. Ten percent of executive job switches trigger about 1.3 analysts to follow them, although the frequency of executive-analyst co-migration climbs to 23 percent for within-industry migration. Both amounts are approximately three times the level of analyst initiation observed for a matched sample without a management change.

Many of the outputs of analyst/managerial interactions are observable to outsiders. We take advantage of this fact to provide greater insight into the attributes that make analysts more likely to initiate coverage conditional on a managerial move. Our evidence suggests that analysts make the decision to initiate coverage following a managerial move based on the intensity of the relationship with the manager, as measured, for example, by time covered or number of private meetings, as well as their ability to accurately forecast the firm's earnings. However, we find that analysts' advantages in covering the executive's origin firm disappear once the executive leaves the firm. The analysts' following of the new firm quickly becomes "average" in most aspects and meetings with management exceed that of the norm. Finally, we provide evidence that co-migrating analysts have increased the overall market capitalization that they cover while still maintaining their average accuracy, suggesting a potential financial benefit for analysts compensated on market capitalization.

We also examine the impact of Reg FD, but find it does not weaken our results. This suggests that the manager/capital market relationships are much deeper than a simple preference in access to information, unlike results documented in prior literature (Cohen et al. 2010).

Our results contribute to the literature's fundamental understanding of manager/capital market relationships by showing that relationships extend beyond the specific firm in which they are created. They also contribute to the growing literatures on the importance of managerial style and the role of social networks by providing dynamic evidence of how such items impact analysts' activities. These results should be of interest to accounting scholars, as the profession seeks to develop an understanding of capital markets beyond what traditional agency paradigms predict. Future research may explore theoretical explanations for the development and survival of relationships across firms.

REFERENCES

- American Heritage Dictionary. 2004. *The American Heritage Dictionary of the English Language*. Fourth edition. Available at: <http://www.thefreedictionary.com/relationship>
- Bamber, L. S., J. Jiang, and I. Y. Wang. 2010. What's my style? The influence of top managers and their personal backgrounds on voluntary corporate financial disclosure. *The Accounting Review* 85 (4): 1113–1162.
- Barniv, R., O.-K. Hope, M. Myring, and W. Thomas. 2009. Do analysts practice what they preach and should investors listen? Effects of recent regulations. *The Accounting Review* 84 (4): 1015–1039.
- Beatty, A., S. Liao, and J. Yu. 2013. The spillover effect of fraudulent financial reporting on peer firms' investments. *Journal of Accounting and Economics* 55 (2): 183–205.
- Bertrand, M., and A. Schoar. 2003. Managing with style: The effect of managers on firm policies. *Quarterly Journal of Economics* 118 (4): 1169–1208.
- Bhojraj, S., C. Lee, and D. Oler. 2003. What's my line? A comparison of industry classification schemes for capital market research. *Journal of Accounting Research* 41 (5): 745–774.

- Bhushan, R. 1989. Firm characteristics and analyst following. *Journal of Accounting and Economics* 11: 255–274.
- Bradshaw, M. 2004. How do analysts use their earnings forecasts in generating stock recommendations? *The Accounting Review* 79 (1): 25–50.
- Bradshaw, M., G. Miller, and G. Serafeim. 2009. *Accounting Choice Heterogeneity and Analysts' Forecasts*. Working paper, University of Michigan.
- Brass, D. J., J. Galaskiewicz, H. R. Greve, and W. Tsai. 2004. Taking stock of networks and organizations: A multilevel perspective. *Academy of Management Journal* 47 (6): 795–817.
- Bushee, B., and G. Miller. 2012. Investor relations, firm visibility, and investor following. *The Accounting Review* 87 (3): 867–897.
- Bushee, B., M. Jung, and G. Miller. 2011. Conference presentations and the disclosure milieu. *Journal of Accounting Research* 49 (5): 1163–1192.
- Cadman, B., S. Klasa, and S. Matsunaga. 2010. Determinants of CEO pay: A comparison of ExecuComp and non-ExecuComp firms. *The Accounting Review* 85 (5): 1511–1543.
- Cohen, L., A. Frazzini, and C. Malloy. 2008. The small world of investing: Board connections and mutual fund returns. *Journal of Political Economy* 116 (5): 951–979.
- Cohen, L., A. Frazzini, and C. Malloy. 2010. Sell side school ties. *Journal of Finance* 65 (4): 1409–1437.
- Collins English Dictionary. 2003. *Collins English Dictionary—Complete and Unabridged*. Available at: <http://www.thefreedictionary.com/relationship>
- Dechow, P., A. Hutton, and R. Sloan. 2000. The relation between analysts' forecasts of long-term earnings growth and stock price performance following equity offerings. *Contemporary Accounting Research*: 1–32.
- DeJong, D., and Z. Ling. 2010. *Managers: Their Effects on Accruals and Firm Policies*. Working paper, The University of Iowa.
- Dyreng, S. D., M. Hanlon, and E. L. Maydew. 2010. The effects of managers on corporate tax avoidance. *The Accounting Review* 85 (4): 1163–1189.
- Fee, C. E., and C. J. Hadlock. 2004. Management turnover across the corporate hierarchy. *Journal of Accounting and Economics* 37 (1): 3–38.
- Francis, J., D. Nanda, and X. Wang. 2006. Re-examining the effects of Regulation Fair Disclosure using foreign listed firms to control for concurrent shocks. *Journal of Accounting and Economics* 41 (3): 271–292.
- Ge, W., D. Matsumoto, and J. Zhang. 2011. Do CFOs have styles of their own? An empirical investigation of the effect of individual CFOs on financial reporting practices. *Contemporary Accounting Research* 28 (4): 1141–1179.
- Groysberg, B., P. Healy, and D. Maber. 2011. What drives sell-side analyst compensation at high-status banks? *Journal of Accounting Research* 49 (4): 969–1000.
- Hambrick, D. C., and P. A. Mason. 1984. Upper echelons: The organization as a reflection of its top managers. *Academy of Management Review* 9: 193–206.
- Hayes, R., and S. Schaefer. 1999. How much are differences in managerial ability worth? *Journal of Accounting and Economics* 27: 125–148.
- Healy, P., A. Hutton, and K. Palepu. 1999. Stock performance and intermediation changes surrounding sustained increases in disclosure. *Contemporary Accounting Research* 16 (3): 485–520.
- Hribar, P., and H. Yang. 2010. *CEO Confidence, Management Earnings Forecasts, and Earnings Management*. Working paper, Cornell University.
- Hwang, B., and S. Kim. 2012. *Earnings Management and Social Ties*. Working Paper, Purdue University.
- Jiang, J., K. Petroni, and I. Wang. 2010. CFOs and CEOs: Who have the most influence on earnings management? *Journal of Financial Economics* 96 (3): 513–526.
- Lin, H., and M. McNichols. 1998. Underwriting relationships, analysts' earnings forecasts and investment recommendations. *Journal of Accounting and Economics* 25 (February): 101–128.
- Mayew, W. J. 2008. Evidence of management discrimination among analysts during earnings conference calls. *Journal of Accounting Research* 46 (3): 627–659.

- McNichols, M., and P. C. O'Brien. 1997. Self-selection and analyst coverage. *Journal of Accounting Research* 35 (Supplement): 167–199.
- Mergenthaler, R., S. Rajgopal, and S. Srinivasan. 2010. *CEO and CFO Career Consequences to Missing Quarterly Analysts Forecasts*. Working paper, Harvard Business School.
- Wang, I. 2007. Private earnings guidance and its implications for disclosure regulation. *The Accounting Review* 82 (5): 1299–1332.
- Westphal, J. D., and M. B. Clement. 2008. Sociopolitical dynamics in relations between top managers and security analysts: Favor rendering, reciprocity, and analyst stock recommendations. *Academy of Management Journal* 51 (5): 873–897.
- Yang, H. 2012. Capital market consequences of managers' voluntary disclosure styles. *Journal of Accounting and Economics* 53: 167–184.

APPENDIX A
Variable Definitions

Panel A: Firm-Level Variables (sources: I/B/E/S, Compustat, CRSP, Thomson-Reuters Mutual Fund Holdings)

Variable	Definition
<i>Analyst Following</i>	Number of analysts who cover ^a the firm in the year prior to the executive's appointment in the destination firm.
<i>ΔAnalyst Following</i>	Number of analysts who cover the destination firm in the year following minus the year before the executive's appointment in the destination firm, scaled by the firm's market capitalization.
<i>Analyst Overlap</i>	Number of analysts who cover ^a the origin and the destination firms in the year prior to the executive's appointment in the destination firm.
<i>Book To Market</i>	Book value of common stockholder equity divided by market value of equity as of the end of the most recent fiscal year preceding the executive's transition.
<i>Firm Size</i>	Natural logarithm of <i>Total Assets</i> of the firm as of the end of the most recent fiscal year preceding the executive's transition, per Compustat.
<i>Migrate</i>	An indicator variable equal to 1 if at least one analyst covering ^a the origin firm for more than one year prior to the executive's departure initiates coverage of the destination firm by the end of the first full fiscal year following the executive's appointment, and 0 otherwise.
<i>Migrate Funds</i>	An indicator variable equal to 1 if at least one mutual fund holding at least 0.0001 percent of the shares of the origin firm prior to the executive's departure starts investing in the destination firm and holds at least 0.0001 percent of the shares by the end of the first full fiscal year following the executive's appointment, and 0 otherwise.
<i>Migrants</i>	Number of analysts from the origin firm who initiate coverage of the destination firm by the end of the first full fiscal year following the executive's appointment.
<i>Recent Turnover</i>	An indicator variable equal to 1 if the origin or destination firm hires or promotes a new CEO or CFO in the prior year, and 0 otherwise.
<i>Same Four-Digit SIC</i>	An indicator variable equal to 1 if the origin and destination firms are in the same four-digit SIC group, and 0 otherwise.
<i>Same Industry</i>	An indicator variable equal to 1 if the origin and destination firms are in the same two-digit SIC group, and 0 otherwise.
<i>Stock Return</i>	Cumulative market-adjusted return over the most recent fiscal year preceding the executive's transition.
<i>Turnover</i>	An indicator variable equal to 1 if a top executive migrates from the origin to the destination firm, and 0 otherwise.

Panel B: Analyst-Level Variables (sources: I/B/E/S, Thomson Research, CRSP)

Variable	Definition
<i>Absolute Forecast Error</i>	The analyst's average annual absolute forecast error for the firm (based on the first annual EPS forecast issued for the current fiscal year) scaled by stock price as of the latest earnings announcement.
<i>Brokerage Overlap</i>	An indicator variable equal to 1 if the brokerage house that employs the analyst who covers the origin firm also covers the destination firm before the executive departure, and 0 otherwise.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
<i>Forecast Horizon</i>	A discrete variable equal to 1, 2, or 3, depending on the longest horizon over which the analyst reports a forecast for the firm (1, 2, or 3 years ahead).
<i>Frequency of Forecast Revisions</i>	The average number of one-year-ahead EPS forecasts issued in a given year by an analyst for a given firm.
<i>Meeting with Management</i>	An indicator variable equal to 1 if the analyst issues a report following a one-on-one meeting with the firm's management, and 0 otherwise. To identify those reports, we conduct a full text search of analyst reports in Thomson Research using the following keyword combinations: (management or company) NEXT (meeting or visit or tour). We then manually check that those reports are written following a one-on-one meeting between the analyst and top management.
<i>Recommendation Profitability</i>	The average profitability of the analyst's recommendations. Profitability for buy and strong buy (sell and strong sell) recommendations is the six-month size-decile adjusted stock return following the recommendation date (multiplied by -1). Hold recommendations are excluded.
<i>School Tie</i>	An indicator variable equal to 1 if the analyst and the departing executive went to the same university (not necessarily at the same time, school, or degree level), and 0 otherwise. Educational background is collected from various sources (BoardEx, LinkedIn, Zoominfo.)
<i>Strong Buy Recommendation</i>	An indicator variable equal to 1 if the average recommendation by the analyst is strictly below 2 on the 1 to 5 I/B/E/S scale (1 = strong buy, 5 = strong sell), and 0 otherwise.
<i>Years of Coverage</i>	The number of years between the first forecast issued by an analyst for a firm and the end of the last full fiscal year of employment for departing executives in origin firms or the end of the first full fiscal year of employment for executives joining destination firms, depending on whether the variable is measured pre- or post-executive migration.
Δ Portfolio Size	The change in the natural logarithm of the total market capitalization of firms covered by an analyst from year -1 to year $+1$, where year 0 is the year in which an executive joins the destination firm.

^a An analyst covers a firm in a given year if (s)he issues at least one forecast for the firm's current fiscal year EPS. For *Analyst Following*, *Firm Size*, *Book To Market*, and *Stock Return*, the suffix *Origin (Destination)* indicates that the variable is measured in the origin (destination) firm, while the suffix *Difference* indicates that the variable is calculated as the difference between the destination firm, and the origin firm (e.g., *Stock Return Difference* is the market-adjusted return in the destination firm minus the market-adjusted return in the origin firm).

Network Ties Among Low-Tax Firms

Jennifer L. Brown

Arizona State University

Katharine D. Drake

The University of Arizona

ABSTRACT: This study examines (1) whether network ties help explain variation in tax avoidance, and (2) how the relation between network ties and tax avoidance varies depending on the nature and context of those ties. We posit that information on a range of tax-avoidance strategies is shared among firms through their social network connections. Using board interlocks to proxy for these connections, we find that firms with greater board ties to *low-tax* firms have lower cash ETRs themselves. Ties to *low-tax* firms are more influential when the focal firm and its network partner are operationally and strategically similar, as are ties created by executive directors. Board ties to *low-tax* firms are also more influential when the focal firm and its network partner engage the same local auditor. Overall, our results suggest that the influence of firms' network ties on their tax-avoidance behavior depends on the character of those ties.

Keywords: *corporate tax avoidance; social networks; director interlocks.*

I. INTRODUCTION

Numerous studies examine whether certain firm characteristics are related to tax avoidance and tax aggressiveness (Desai and Dharmapala 2006; Gupta and Newberry 1997; Lisowsky 2010; Rego 2003; Wilson 2009). We propose that existing explanations of the cross-sectional variation in corporate tax avoidance are incomplete because they do not consider the social structure in which tax-avoidance activity is embedded.¹ Our study ties the literature on tax avoidance together with a robust literature regarding the influence of social networks on firm behavior. We posit that knowledge about tax-savings strategies and experience implementing these

We thank Charles Christian, Scott Dyreng, David A. Guenther (editor), Susan Gyeszly, Michelle Hanlon, Steve Kaplan, Stephen Lusch, Michal Matějka, Melissa Martin, two anonymous referees, the Arizona State University Tax Readings Group, workshop participants from the 2011 JATA Conference, The University of Arizona, University of Houston, and Arizona State University for their helpful comments and suggestions.

Editor's note: Accepted by David A. Guenther.

Submitted: June 2011

Accepted: September 2013

Published Online: November 2013

¹ Social structure refers to the pattern of social relations, formal or informal, among members of a social system (Rogers 2003). Granovetter (2005) posits that social structure affects economic outcomes through its effect on the quality and flow of information among members of the system.

strategies is shared among firms via their network ties. Using board interlocks as a proxy for network ties, we find that a firm's tax-avoidance behavior is related to its ties to *low-tax* firms, those firms ranked in the lowest quintile based on industry-adjusted, long-run cash effective tax rates (ETRs). Further, we predict and find that the influence of network ties on tax avoidance depends on the character and context of the firm's ties.

Network ties influence corporate behavior because they promote the diffusion of new ideas and allow networked firms to learn vicariously from each other's experiences (e.g., Bizjak, Lemmon, and Whitby 2009; Davis and Greve 1997; Haunschild 1993). Drawing on diffusion theory, Brown (2011) finds evidence that board interlocks affect the discrete tax policy decision at the focal firm involving the corporate-owned life insurance tax shelter. However, she does not consider the ultimate performance effects of board interlocks. Building on the small-sample evidence in Brown (2011), we examine (1) whether network ties help explain variation in tax avoidance generally, and (2) how the relation between network ties and tax avoidance varies depending on the nature and context of those ties.

As part of a growing body of literature on corporate tax avoidance, Dyreng, Hanlon, and Maydew (2008) identify a set of firms that are able to achieve significantly low cash ETRs over long periods of time and call for additional research on the nature of these long-term tax avoiders. We examine tax avoidance in the context of social networks and find evidence that, all else equal, firms with greater ties to *low-tax* firms exhibit lower cash ETRs themselves.

The influence of network ties on firm behavior may depend on characteristics of both the firm's ties to other firms and the type of knowledge being shared across those ties (Adler and Kwon 2002; Argote, McEvily, and Reagans 2003; Reagans and McEvily 2003). Therefore, once we establish a basic network effect for tax avoidance, we adopt a contingency approach and explore which interlocks play a stronger role in the tax setting.

First, we expect a firm's ties to operationally and strategically similar network partners to be more influential. Knowledge shared by similar network partners is likely to be relevant and result in strategic advice that the focal firm can readily utilize. We also expect ties created by executive directors to be more influential because these directors have greater first-hand knowledge, status, and power to influence corporate decision-making. Finally, prior research suggests that an indirect link to a third party, like a common auditor, can amplify the effect of a direct link between firms (Gulati and Westphal 1999). Thus, we expect board ties to firms that also engage the same auditor in the same practice area (common local auditor) to be more influential.

We find that a firm's ties to similar network partners are more influential than ties to dissimilar network partners when we capture partner similarity using measures of income mobility, growth, and risk. However, we find no statistical difference in comparing the effect of intra-industry ties relative to extra-industry ties. Overall, our results are consistent with our hypothesis; with respect to tax knowledge, network ties seem to be more influential when those ties are likely to provide relevant information about strategies that can be implemented at the focal firm.

Our results also indicate that network ties created by executive directors have more influence on tax avoidance relative to ties created by non-executive directors. Finally, we find that board ties to *low-tax* firms are more influential when the focal firm and its network partner engage the same local auditor, consistent with theory on the role of multifaceted ties. Together, our findings suggest that the nature and context of a firm's network ties play a role in the influence of those ties on the firm's tax-avoidance behavior.

In recent work, Larcker, So, and Wang (2013) provide evidence that board of director connections have an economic impact on the firm; better-connected firms earn significantly higher risk-adjusted returns and experience higher gains in profitability. One alternative explanation for our results could be that board connections provide a conduit for information on overall strategy and operating practices, and low tax rates are simply a consequence of these other shared practices.

Endogenous matching is a related concern; high-quality directors may be attracted to better performing firms and *vice versa*.

We perform several tests to examine whether these alternative explanations are driving our main results. We find no evidence that network ties to firms that perform better in terms of profitability, gross margins, or returns are associated with low cash ETRs at the focal firm. Further, we demonstrate that the network effect on tax avoidance is asymmetric; cash ETR at the focal firm is associated with the focal firm's *low-tax* ties but not its *high-tax* ties. Thus, it is unlikely that *low-tax* ties are merely a proxy for ties to better-performing firms or that the tax effect we identify is merely a consequence of other shared practices. We also show that firms that acquire ties to *low-tax* network partners experience a decrease in their cash ETRs relative to those that do not. While these tests mitigate concerns that our results are driven by some correlated omitted variable and suggest that the network effect we identify is likely driven by shared knowledge of, and experience related to, tax-avoidance strategies, we acknowledge that we cannot fully rule out other causal scenarios.

This study contributes to the accounting literature in several ways. First, this study ties the tax-avoidance and social network literatures together and directly responds to the call in Hanlon and Heitzman (2010) for research on the role of directors and their ties to other tax-avoidance firms. We find evidence that network ties in the form of board interlocks are associated with corporate tax avoidance, consistent with network ties serving as conduits for the flow of tax-savings strategies.

Second, this study introduces theory and evidence on the contingent nature of network ties to the accounting literature. Building on prior research in management (e.g., Haunschild and Beckman 1998; Carpenter and Westphal 2001), we posit that not all network ties have an equal effect on corporate tax behavior. In the tax setting, we expect and find that ties facilitating the transfer of tacit and complex knowledge are particularly beneficial. Our results highlight the importance of considering both the nature of the information being transferred and the nature of a firm's ties when studying the impact of social networks on firm behaviors.

Finally, our results extend the findings in Brown (2011). Evidence that a particular strategy or set of strategies diffuses across board interlocks leaves open the question of whether social network influences will affect performance outcomes (Mizruchi and Marquis 2006). In this study, we shift focus so that our behavior of interest is not a single, discrete, firm-level decision as in Brown (2011), but rather a quantitative outcome that we measure on a continuous scale (Mizruchi and Marquis 2006). By considering the performance implications of directors' experiences on other boards, we add to an under-explored area of board interlock research (McDonald, Westphal, and Graebner 2008).

II. HYPOTHESIS DEVELOPMENT

The literature relating board interlocks and corporate practices spans from early studies on the diffusion of "poison pills" (Davis 1991) and the adoption of organizational structures (Palmer, Jennings, and Zhou 1993) to more recent research regarding the spread of stock option backdating practices (Bizjak et al. 2009) and earnings management behavior (Chiu, Teoh, and Tian 2013). As a suggestion for future research on tax avoidance, Hanlon and Heitzman (2010) propose that researchers consider finance and management studies of networks and examine the impact of directors and their social connections to other firms on tax-avoidance behavior. They highlight the following anecdotal evidence that an interlocked board member was influential in spreading the use of an installment sales tax shelter:

When Merrill Lynch was promoting its now-famous installment sales shelter in the early 1990s, Merrill board member Robert Luciano, who was then chairman of Schering-Plough, took the idea to executives of AlliedSignal, on whose board he also sat. Schering used the scheme too (Novack and Saunders 1998).

Examining diffusion theory in a tax-reporting context, Brown (2011) finds small-sample evidence that the use of the corporate-owned life insurance shelter spread via board interlocks. We seek to expand on this evidence and investigate (1) whether Brown’s (2011) findings related to the diffusion of a single corporate practice generalize such that network ties explain part of the observed variation in corporate tax avoidance, and (2) how the influence of network ties on tax avoidance might vary with the nature and context of those ties.

There are two main reasons why the results in Brown (2011) may not extend to our setting. First, different strategies follow different diffusion patterns and diffuse across different channels (Rogers 2003). For example, Davis and Greve (1997) find that corporate governance practices concerning poison pills appear to have spread via board interlocks, while corresponding practices concerning golden parachutes appear to have spread geographically. Consequently, Brown’s (2011) findings concerning the spread of corporate-owned life insurance may not generalize to a broad set of tax-avoidance strategies. Second, while Brown (2011) finds evidence that board interlocks influence the adoption of a corporate tax practice, she does not examine whether the effect of social networks translates into differences in performance outcomes.²

Dyreng et al. (2008) identify a set of firms that achieve low tax rates over long periods of time. They offer basic descriptive statistics on these firms and call for more research on the characteristics of these firms. We hypothesize that these tax-avoiding firms are privy to knowledge about tax-savings strategies and/or have experience employing tax-savings strategies and that this knowledge and experience is shared via firms’ social networks. Firms could be exposed to new tax-savings techniques through their network ties or could be persuaded to adopt a particular tax-savings strategy after learning that a network partner has had success employing that strategy.³ Robinson, Sikes, and Weaver (2010) find that tax avoidance is related to whether a firm views its tax department as a profit center or as a cost center. Like knowledge of a specific tax strategy, structuring the tax department as a profit center could also be diffused across network ties.

Board interlocks have been shown to influence a variety of corporate strategies, including the adoption of tax shelters. Furthermore, Strang and Soule (1998) note that board interlocks are a readily observable proxy for a firm’s broader set of social network ties. Therefore, using board interlocks to proxy for firms’ network ties, we posit a network effect for tax avoidance. Specifically, we expect to find an association between the focal firm’s board ties to *low-tax* firms and its tax-avoidance behavior:

H1: Network ties to *low-tax* firms will be positively related to tax avoidance at the focal firm.

A less-developed literature on board interlocks examines the contingencies that determine which interlocks are influential in the diffusion of corporate practices (Borgatti and Foster 2003). Adopting a contingency approach, we hypothesize that the influence of network ties on tax avoidance depends on the nature and context of the firm’s network ties. Specifically, we examine the influence of network ties across three settings: (1) ties to similar/dissimilar network partners, (2) ties formed by executive/non-executive directors, and 3) ties to network partners who share a common local auditor.

Ties to Similar and Dissimilar Network Partners

In his strength-of-weak-ties theory, Granovetter (1973) posits that weak ties are particularly beneficial because they provide an actor with access to novel information, whereas strong ties

² The efficacy of an adopted innovation likely varies across firms. Moreover, the cost/benefit trade-off decision faced by early and late adopters of a strategy likely differs (Westphal, Gulati, and Shottell 1997).
³ Diffusion theory concerns the spread of innovations, where an innovation is not limited to an objectively “new” practice, but relates to any practice that is “new” to the firm considering its adoption. Furthermore, the “newness” of an innovation should be perceived broadly in terms of knowledge, persuasion, or a decision to adopt (Rogers 2003).

simply expose an actor to redundant information. Likewise, Burt (1992) suggests that the key to a beneficial social network is having ties that provide additive, non-redundant information. These theories suggest that firms looking for a competitive edge and alternative strategies may benefit from board interlocks with firms following different strategies and operating in different business environments since these dissimilar network partners are more likely to provide directors with novel ideas (Carpenter and Westphal 2001).

However, Hansen's (1999) work indicates that the effect of a tie on knowledge sharing depends on the type of knowledge being shared. Strong ties, like those to similar network partners, play an especially important role when the knowledge being transferred across the social network is tacit and complex (Hansen 1999).⁴ Indeed, prior research indicates that diffusion of strategies is amplified when firms are tied to others with similar organizational or strategic characteristics because adopting firms find the experiences of similar network partners more relevant (Haunschild and Beckman 1998; Davis and Greve 1997; Carpenter and Westphal 2001; Brass, Galaskiewicz, Greve, and Tsai 2004). Similar background knowledge and similar capabilities positively impact a firm's ability to absorb and utilize newly acquired knowledge (Rindfleisch and Moorman 2001).

In addition, researchers have suggested that rather than influencing adoption of practices by spreading awareness of new strategies, board interlocks are apt to play a role in the diffusion of business practices by spreading tacit information on the costs and benefits of adopting practices (Brass, Butterfield, and Skaggs 1998; Strang and Soule 1998). Given the likely role of directors in the diffusion process, in conjunction with the complex nature of information on tax-savings strategies, we expect ties to similar network partners to have more influence relative to those of dissimilar network partners. Overall, we hypothesize:

H2: The effect of network ties on tax avoidance at the focal firm will be stronger when network partners are similar.

Ties Formed by Executive Directors

We distinguish executive interlocks, which we define as those interlocks created when the connecting director is a CEO or CFO of either firm, from non-executive interlocks. Prior research suggests that executive interlocks may have a stronger effect on the diffusion of innovations, relative to non-executive interlocks, because the directors creating executive interlocks are more willing and able to serve as representatives for the firms they connect (Palmer, Barber, Zhou, and Soysal 1995; Palmer et al. 1993; Haunschild 1993).

Dyreng, Hanlon, and Maydew (2010) find evidence of a significant manager fixed-effect for tax avoidance, establishing a link between executives and corporate tax avoidance. Recent studies also link corporate tax-reporting behavior with executives via their compensation (Armstrong, Blouin, and Larcker 2012; Rego and Wilson 2012). Although there have been calls for greater involvement of the entire board in monitoring tax matters (Shulman 2009), we expect that relative to non-executive directors, executives are more likely to have first-hand experience and fine-grained information about specific tax strategies. Theory predicts that this type of first-hand experience is more influential in transferring knowledge and persuading others than second-hand experience (Westphal, Seidel, and Stewart 2001).

Borgatti and Cross (2003) note that expert status is important in predicting the successful transfer of knowledge across actors. Moreover, an executive's formal position within a firm's

⁴ A single, common conceptualization of "strong ties" does not exist in the literature (see Rindfleisch and Moorman 2001). Rather than take an all-encompassing approach, we focus on network partner similarity, a dimension of tie strength we believe is applicable at the inter-organizational level.

hierarchy may amplify the impact of his/her network ties to other firms on the focal firm's strategic decisions. Since the top management team ultimately makes decisions about firm operations, there is likely more value in the information that comes to the firm through its network ties created by these executives (Brass et al. 2004). Thus, we expect:

H3: The effect of network ties on tax avoidance at the focal firm will be stronger for ties formed by executive directors than for non-executive directors.

Ties via Shared Auditor

In this study, we proxy for firms' network ties using board of director interlocks because these ties are observable and commonly used in the social network literature. However, firms have many different types of network ties, as discussed in Section V. Multiplexity, the extent to which two firms are simultaneously linked via multiple types of ties, is another potentially important contextual factor (Beckman and Haunschild 2002; Carpenter, Li, and Jiang 2012). Gulati and Westphal (1999) posit that indirect ties, like those to a mutual third party, can amplify the effect of direct ties. With this in mind, we focus on the additional indirect ties created when two interlocked firms share the same auditor in the same practice area, which we refer to as a common local auditor.

A common local auditor may possess superior knowledge of available tax-planning opportunities (McGuire, Omer, and Wang 2012). Moreover, sharing a common local auditor may increase trust, which can affect the extent of knowledge disclosure and sharing between two parties and reduces apprehension about the veracity of the knowledge being shared (Argote et al. 2003). Trust is particularly important in the transfer of tacit knowledge because tacit knowledge is subjective and intertwined with the experience of the individual sharing the knowledge (Levin and Cross 2004).

Interlocks with network partners that also share a common local auditor are likely to be more influential because, as a common third party, auditors bring expertise, legitimacy, and trust to the interlock relationship. Further, firms should be more influenced by their relationships to network partners when those relationships are multifaceted; tie multiplexity should help resolve uncertainty, increase perceived trust and reliability, and, thus, facilitate the transfer of knowledge (Reagans and McEvily 2003; Beckman and Haunschild 2002). Thus, we hypothesize:

H4: The effect of network ties on tax avoidance at the focal firm will be stronger when the network partners also share a tie to a common local auditor.

III. SAMPLE CONSTRUCTION AND VARIABLE MEASUREMENT

Sample Construction

Our sample begins with all U.S. incorporated firms in the Compustat database between 1996 and 2009.⁵ In the majority of our analyses, we divide our sample into 12 rolling three-year periods. We eliminate observations lacking data to calculate averages of all variables over a three-year period. We exclude firms with negative three-year sum of cash taxes paid, or negative three-year sum of pre-tax income as these firms likely have different objectives, needs, and opportunities for tax planning. We also winsorize average *CETR3* values at 1 to mitigate the effect of outliers. To construct our measure

⁵ Consistent with Dyreng et al. (2008), we eliminate non-corporate entities by excluding SIC code 6798 (Real Estate Investment Trusts) and firms with six-digit CUSIPs ending in "Y" or "Z."

of network ties, we use detailed information on board of director membership from RiskMetrics. After merging firms with the necessary data from both Compustat and RiskMetrics, our final sample has 13,784 firm-period observations consisting of 2,270 unique firms.

Measuring Tax Avoidance

We hypothesize that tax-planning ideas and experiences are shared among firms via network ties. While theory on the diffusion of strategies suggests that network ties are most important when the level of uncertainty surrounding a strategy is high (i.e., tax-shelter-type strategy), information sharing likely encompasses a broad spectrum of planning techniques. Therefore, we use a three-year average measure of cash ETR as our dependent variable as follows:⁶

$$CETR3_{i,p} = \sum_{t=1}^3 (TXPD_{it}) / \sum_{t=1}^3 (PI_{it} - SPI_{it}),$$

where i indicates firm i , $i = 1$ to 2,270; p indicates one of 12 rolling three-year periods within our sample timeframe, 1996–2009, $TXPD$ equals cash taxes paid, PI equals pre-tax income, and SPI equals special items.

Measuring Network Ties

We use board interlocks to proxy for a firm's network ties to *low-tax* firms. Our primary explanatory variable is *L-T TIES*, the number of board ties the firm has to *low-tax* firms, scaled by the firm's total board ties.⁷ In each period, we identify *low-tax* firms as those firms ranked in the lowest quintile based on *CETR3_IND*, the firm's three-year average cash ETR adjusted for the industry median.^{8,9}

From the director information contained within the RiskMetrics database, we identify board interlocks in each three-year period. We intentionally choose not to restrict our measure of network ties to board interlocks within one particular year to avoid losing connections resulting from fiscal year-ends. Additionally, directorships often result from relationships and connections built over a number of years. An incoming director likely had connections within the board in the year or years preceding his/her appointment. Thus, we designate firm i as interlocked with firm j in period p if one or more directors are listed on both boards in any of the three years within the period.

We use data on the individual directors of each firm in the sample to construct an $n \times n$ matrix for each three-year period p , where n equals the number of firms in the sample in that period. If firm

⁶ Consistent with Dyreng et al. (2008), we consider tax avoidance as a measure of a firm's ability to pay a low amount of cash income taxes relative to earnings. We do not infer or imply that these firms are able to achieve a low cash tax rate by illegal or improper means, but rather that they are able to report and pay a lower percentage of earnings in taxes. Concerned that our measure of low tax may be driven by firms engaging in upward earnings management, in untabulated results we replace our measure of tax avoidance with taxes paid scaled by operating cash flows [$TXPD/(OANCF + TXPD)$]. We continue to find that tax avoidance at the focal firm is negatively associated with ties to *low-tax* firms.

⁷ We do not capture a firm's ties to firms outside our sample, but we expect that the ties identified within the RiskMetrics sample are representative of a firm's overall ties and do not expect an omission of additional ties to firms outside the sample to bias our results.

⁸ We classify industries according to Barth, Beaver, and Landsman (1998).

⁹ Since we are interested in identifying firms with strategic knowledge and experience, we construct our measure of *low-tax* to capture how successful a firm is at tax avoidance relative to its industry peers. While we specify *low-tax* as low relative to the industry median, our primary results are robust to using a ranking of *CETR3* without industry adjusting.

TABLE 1
Mean Comparisons Among Quintiles of Three-Year, Cash ETRs (CETR3)

	<i>n</i>	<i>CETR3</i>	<i>GETR3</i>	<i>SIZE3</i>	<i>ROA3</i>	<i>GP3</i>	<i>RET3</i>	<i>Low-tax ties (raw)</i>	<i>TIES</i>	<i>L-T TIES</i>
<i>Rank CETR3 = 1</i>	2,753	0.0880	0.3200	7.5423	0.0888	0.4258	1.1770	0.9938	4.6058	0.1841
<i>Rank CETR3 = 2</i>	2,758	0.1943	0.3343	7.6931	0.1029	0.3922	0.9189	1.1138	5.5076	0.1822
<i>Rank CETR3 = 3</i>	2,762	0.2607	0.3515	7.5109	0.1171	0.3769	0.9139	0.9819	5.1849	0.1633
<i>Rank CETR3 = 4</i>	2,758	0.3206	0.3688	7.3520	0.1284	0.3560	0.8479	0.8993	4.7851	0.1563
<i>Rank CETR3 = 5</i>	2,753	0.4664	0.3848	7.2823	0.0989	0.3496	0.6392	0.8624	4.5194	0.1583

This table presents descriptive statistics, comparing quintiles of three-year Cash ETRs. Each of the variables is constructed as an average over a three-year period. There are 12 rolling three-year periods in our sample timeframe (1996–2009).

All variables except *CETR3*, *GETR3*, and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3* or *GETR3*, and observations with *CETR3* or *GETR3* greater than 1 are winsorized at 1.

See Appendix A for complete variable definitions.

i and firm *j* share one or more directors, the *ij*th element of the matrix is coded as 1, otherwise the *ij*th element is coded as 0.¹⁰ Separately, we divide firm-period observations into quintiles based on *CETR3_IND* and construct an $n \times 1$ matrix where the *i*th element is coded as 1 if firm *i* is in the lowest quintile of *CETR3_IND*, and 0 otherwise. We multiply the $n \times n$ matrix by the $n \times 1$ matrix to determine the number of *low-tax* firms each firm is tied to during the period. Finally, we follow prior social network literature (Bonacich 1972; Friedkin 1991; Leenders 2002) and scale this measure by the firm’s total number of board ties to arrive at *L-T TIES*.

Table 1 presents descriptive statistics for our sample partitioned into quintiles based on *CETR3*. Firms in the lowest quintile (*Rank CETR3* = 1) have a three-year average cash ETR of less than 10 percent, while those in the highest quintile (*Rank CETR3* = 5) have a three-year average cash ETR of over 45 percent. As the data in Table 1 indicate, profitability (*ROA3*), operating margins (*GP3*), and market returns (*RET3*) vary with the level of tax avoidance (*Rank CETR3*). However, there does not appear to be an obvious and consistent relation between *low-tax* and financially well-managed firms as captured by these three metrics. We perform additional tests to assess whether our results are driven by alternative explanations involving ties to financially well-managed firms and report the results of these tests in Section V.

The right three columns of Table 1 show *Low-tax ties (raw)*, the number of *low-tax* firms to which the focal firm is connected, *TIES*, the total number of firms to which the focal firm is connected, and *L-T TIES*, which is the ratio of the two and our variable of interest. On average, within the lowest cash ETR quintile, 18.41 percent of a firm’s total ties are to *low-tax* firms. In contrast, within the highest cash ETR quintile, the average percentage of ties to *low-tax* firms is smaller at 15.83 percent ($p < 0.01$). Overall, the partitions in Table 1 are consistent with H1 and indicate that *low-tax* firms are tied disproportionately to each other via board interlocks.

¹⁰ Consequently, our interlock measure is the same whether firm *i* and firm *j* share only one director or share several common directors. This design choice makes the interlock identification problem tractable, but discounts the additional influence that multiple shared directors might have on firm behavior.

IV. PRIMARY ANALYSIS

To test H1 in a multivariate setting, we estimate the following OLS model:¹¹

$$\begin{aligned} CETR3_{i,p} = & \beta_0 + \beta_1 L\text{-}T\ TIES_{i,p} + \beta_2 SIZE3_{i,p} + \beta_3 ROA3_{i,p} + \beta_4 LEV3_{i,p} + \beta_5 PPE3_{i,p} \\ & + \beta_6 INTAN3_{i,p} + \beta_7 RD3_{i,p} + \beta_8 AD3_{i,p} + \beta_9 NOL3_{i,p} + \beta_{10} FOR3_{i,p} + \beta_{11} SPI3_{i,p} \\ & + industry\ controls + period\ controls + \varepsilon, \end{aligned} \tag{1}$$

where the independent and dependent measures are described below and calculated as defined in Appendix A.

H1 predicts a positive relation between network ties to *low-tax* firms and tax avoidance at the focal firm. However, because we capture tax avoidance as *CETR3*, the firm’s three-year cash ETR, we expect a negative coefficient on our explanatory variable, *L-T TIES*. We include various firm characteristics to control for other determinants of tax avoidance. Like our dependent variable, we measure these control variables as three-year averages.

First, we include a measure of firm size (*SIZE3*) because larger firms have access to tax-planning strategies that might exhibit economies of scale (Rego 2003), and size is a proxy for tax-planning sophistication (Hanlon, Mills, and Slemrod 2005). We also control for leverage (*LEV3*), capital intensity (*PPE3*), intangible assets (*INTAN3*), research and development activities (*RD3*), and foreign income (*FOR3*) because prior research suggests that firm complexity and firm operations are associated with varying opportunities to avoid taxes (Gupta and Newberry 1997; Rego 2003; S. Chen, X. Chen, Cheng, and Shevlin 2010).¹²

Hanlon and Slemrod (2009) suggest that some firms may be less likely to avoid taxes because they face high political costs from doing so; therefore, we include a control for advertising expense (*AD3*) to capture a firm’s visibility. We control for profitability (*ROA*) and include an indicator variable equal to 1 if the firm has a net operating loss (*NOL3*) reported in the year before or any year within the three-year period. Firms with low profitability and/or net operating losses likely have less incentive to engage in tax avoidance. Additionally, we follow Dyreng et al. (2008) and exclude special items from the denominator when calculating *CETR3*. However, since special items may affect a firm’s taxes, we include special items (*SPI3*) as a separate control variable.¹³

Descriptive Statistics

Table 2 presents descriptive statistics for our sample and compares our sample to the broader set of Compustat firms. When compared to the general population of Compustat firms, our sample firms, which are limited to firms covered by RiskMetrics, are larger, more profitable, and have lower three-year cash ETRs. Relative to the average Compustat firm, the mean GAAP ETR is

¹¹ We also considered testing our hypotheses using a network autocorrelation model of the form: $CETR3\_IND = \alpha + \rho W(CETR3\_IND) + X\beta + \varepsilon$, where $W(CETR3\_IND)$ represents the weighted sum of the cash ETRs of the focal firm’s network partners and is derived from the same $n \times n$ adjacency matrix of direct board interlocks we use in our main analysis. However, specifying the weighted matrix W to test for the contingency effects we hypothesize in H2–H4 would be complex, requiring *a priori* assumptions about the how much influence one type of tie or network partner would exert versus another type of tie or partner (Leenders 2002). The research design we choose allows us to test whether various types of ties are more influential without explicitly assuming a particular weight.

¹² The Compustat variable for foreign income, PIFO, is often missing or inaccurate. In alternate tests, we include an indicator variable equal to 1 if the firm reported any foreign income in our three-year period. The sign and significance of our variables of interest remain unchanged.

¹³ We include industry and period controls and cluster standard errors by firm and period. However, the sign and significance of our variables of interest are not sensitive to these design choices.

TABLE 2
Descriptive Statistics

	All Compustat Firms n = 27,760			Sample Firms n = 13,784		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
CETR3	0.2758	0.2678	0.1737	0.2660	0.2607	0.1488
GETR3	0.3497	0.3603	0.1620	0.3519	0.3583	0.1414
SIZE3	6.1540	6.1031	2.0366	7.4762	7.2938	1.5243
ROA3	0.1029	0.0855	0.0754	0.1072	0.0926	0.0680
LEV3	0.1663	0.1375	0.1560	0.1800	0.1700	0.1419
PPE3	0.4909	0.4011	0.3788	0.5157	0.0977	0.3609
INTAN3	0.1401	0.0699	0.1713	0.1595	0.0000	0.1737
RD3	0.0288	0.0000	0.0696	0.4444	0.0000	0.4969
AD3	0.0099	0.0000	0.0282	0.0108	0.0000	0.0276
NOL3	0.0019	0.0000	0.0432	0.0014	0.0000	0.0371
FOR3	0.0118	0.0000	0.0288	0.0169	0.0000	0.0295
SPI3	0.0001	0.0000	0.0016	-0.0111	-0.0029	0.0336

This table presents descriptive statistics and compares our sample firm-period observations to the broader sample of Compustat firms. Each of the variables is constructed as an average over a three-year period. There are 12 rolling three-year periods in our sample timeframe (1996–2009).

All variables except *CETR3*, *GETR3*, and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3*, and observations with *CETR3* or *GETR3* greater than 1 are winsorized at 1.

See Appendix A for complete variable definitions.

higher for our sample firms, but the median is lower. In sensitivity analysis described in Section V, we perform similar tests using GAAP ETRs to measure tax avoidance but do not find an association between firms’ GAAP ETRs and their network connections. Since our sample firms are generally larger, they likely have larger boards and more interlocked directors relative to the broader sample of Compustat firms. If so, the variance in our primary measure, *L-T TIES* is likely lower, biasing against finding our hypothesized results.

Ties Among Low-Tax Firms

Table 3 reports the results from estimating Equation (1) above.¹⁴ Consistent with H1, the coefficient on *L-T TIES* is negative and marginally significant ($p = 0.06$).¹⁵ All else equal, firms with greater ties to *low-tax* firms exhibit greater tax avoidance themselves as reflected in lower cash ETRs. The coefficients on our control variables are generally significant and consistent with prior

¹⁴ Since our data are an unbalanced panel that includes the same firm across multiple periods, we cluster standard errors by firm and period to correct for serial correlation. A single firm-year observation could enter the regression multiple times because our dependent and independent variables are calculated as rolling three-year averages. To address concerns that our results are sensitive to this, we also run our regressions using only non-overlapping three-year measures and continue to find a significant negative association between *L-T TIES* and a firm’s three-year cash ETR.

¹⁵ In untabulated results, we separate our independent variable, a ratio, into its numerator, *low-tax ties*, and denominator, *TIES*, and re-estimate Equation (1). We find that the coefficient on the unscaled number of ties to *low-tax* firms is negative and significant, while the coefficient on total network ties, *TIES*, is positive and marginally significant.

TABLE 3

OLS Regression of Tax Avoidance (*CETR3*) on a Firm’s Ties to *Low-tax* Firms (*L-T TIES*)

$$\begin{aligned} CETR3_{i,p} = & \beta_0 + \beta_1 L-T\ TIES_{i,p} + \beta_2 SIZE3_{i,p} + \beta_3 ROA3_{i,p} + \beta_4 LEV3_{i,p} + \beta_5 PPE3_{i,p} \\ & + \beta_6 INTAN3_{i,p} + \beta_7 RD3_{i,p} + \beta_8 AD3_{i,p} + \beta_9 NOL3_{i,p} + \beta_{10} FOR3_{i,p} + \beta_{11} SPI3_{i,p} \\ & + industry\ controls + period\ controls + \varepsilon. \end{aligned}$$

(1)

	Predicted Sign	Coefficient	t-statistic
<i>L-T TIES</i>	—	−0.016	−1.93*
<i>SIZE3</i>	?	−0.005	−2.65***
<i>ROA3</i>	?	−0.169	−3.75***
<i>LEV3</i>	?	−0.137	−5.28***
<i>PPE3</i>	—	−0.007	−0.81
<i>INTAN3</i>	—	0.039	1.34
<i>RD3</i>	—	−0.014	−2.40**
<i>AD3</i>	+	−0.015	−0.24
<i>NOL3</i>	—	0.008	0.24
<i>FOR3</i>	—	−0.045	−0.45
<i>SPI3</i>	+	0.317	2.02**
Intercept		Yes	
R ²		7.20%	

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.
n = 13,784.
All regressions include industry and period controls, and standard errors are clustered by firm and by period.
All variables except *CETR3* and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3*, and observations with *CETR3* greater than 1 are winsorized at 1.
See Appendix A for complete variable definitions.

findings. Firms that are larger, more profitable, and more highly leveraged have lower three-year cash ETRs, as do firms with greater research and development expenditures and fewer special items.

The results in Table 3 support H1 and are consistent with the notion that firms benefit from knowledge shared via their network ties to other *low-tax* firms. Equation (1) assumes that all *L-T TIES* are equally influential and the coefficient on *L-T TIES* reported in Table 3 represents the average effect of having a tie to a *low-tax* network partner on tax avoidance at the focal firm. To test H2–H4, we relax this assumption and regress tax avoidance on various types of *L-T TIES*.

Ties to Similar and Dissimilar Network Partners

To test H2 we distinguish ties to *low-tax* firms that are similar to the focal firm, *L-T TIES_{sim}*, from ties to *low-tax* firms that are dissimilar from the focal firm, *L-T TIES_{dissim}*. H2 posits that board ties to *low-tax* firms that are operationally and strategically similar to the focal firm should be more influential because they are likely to provide more relevant information that the focal firm can absorb and utilize. Since we expect the coefficient on *L-T TIES_{sim}* to be negative, a finding that the coefficient on *L-T TIES_{sim}* is more negative than the coefficient on *L-T TIES_{dissim}* ($\beta_{L-T\ TIES_{sim}} < \beta_{L-T\ TIES_{dissim}}$) would support H2.

Our first proxy for network partner similarity is based on industry membership (Haunschild and Beckman 1998; Carpenter and Westphal 2001). Intra-industry contacts share the same operating environment. Moreover, some tax preferences are specific to particular industries. We construct $L-T\ TIES\ IND_{sim}$ as the number of *low-tax* firms the focal firm is connected to within its industry and $L-T\ TIES\ IND_{dissim}$ as the number of *low-tax* firms the focal firm is connected to outside of its industry.¹⁶

Shared industry membership is a relatively broad measure of network partner similarity. To sharpen our focus on operational and strategic similarity, we examine similarity across income mobility, growth, and risk, three additional dimensions that may be specifically important in transferring and utilizing tax knowledge. In these tests, we consider a network tie to be similar if the focal firm and the network partner exhibit similar levels of the variable of interest (i.e., high/high or low/low).¹⁷ We code a tie as dissimilar when the network partner and the focal firm exhibit dissimilar levels of the variable of interest (i.e., high/low or low/high). We label all other ties, those that are not clearly similar or dissimilar, as neutral ties. We make no predictions regarding the impact of neutral ties.

Dyreng and Lindsey (2009) find that multinational firms have lower effective tax rates, and recent research has highlighted that multinational firms are generally “income mobile,” meaning that they are able to strategically shift income among jurisdictions (De Simone and Stomberg 2012). Following De Simone and Stomberg (2012), we measure *IncomeMobile* based on period ranks of research and development expense, advertising expense, foreign sales percentage, gross profit percentage, and certain industries known to be income mobile. We classify firms with measures of *IncomeMobile* in the top 20 percent (bottom 20 percent) of the sample distribution for the period as “high” (“low”) income mobile firms. Then we use these high and low groups to construct $L-T\ TIES\ IncomeMobile_{sim}$, $L-T\ TIES\ IncomeMobile_{dissim}$, and $L-T\ TIES\ IncomeMobile_{neutral}$.

Firms with differing growth opportunities likely face different resource constraints and different incentives related to tax planning (Chen et al. 2010), consistent with evidence from Drake (2013) that growth firms have relatively higher levels of book-tax differences. Using book-to-market ratio ($CEQ/PRCC\_F * CSHO$) as a proxy for growth opportunities, we classify firms in the top 20 percent (bottom 20 percent) of the sample distribution of book-to-market for the period as “high” (“low”) growth. Then we use these high and low groups to construct $L-T\ TIES\ Growth_{sim}$, $L-T\ TIES\ Growth_{dissim}$, and $L-T\ TIES\ Growth_{neutral}$.

Our final measure of similarity incorporates risk and is designed to specifically capture strategic similarity. Prior research indicates that ideas are likely to diffuse among firms with similar strategic agendas and similar beliefs about the actions necessary to take in order to realize those agendas (O’Neil, Pouder, and Buchholtz 1998). Dhaliwal, Lee, Pincus, and Steele (2012) demonstrate an association between firm risk and taxable income. In the extreme, Kim, Li, and Zhang (2011) find an association between tax avoidance and crash risk. We classify a firm as “high” risk if the firm’s measures for three or more of the following risk metrics are above the sample median for the period: three-year standard deviation of returns, three-year standard deviation of cash flows (OANCF), three-year standard deviation of ROA, and three-year average beta, leverage, or Z-score. We classify firms with three or more metrics below the sample median for the period as “low” risk. Using these high- and low-risk groups, we then construct $L-T\ TIES\ Risk_{sim}$, $L-T\ TIES\ Risk_{dissim}$, and $L-T\ TIES\ Risk_{neutral}$.

¹⁶ While the Clayton Act expressly prohibits director interlocks among direct competitors, it does not necessarily prohibit intra-industry interlocks (Mizruchi 1996). We employ an industry classification schema from Barth et al. (1998) that has 15 broad categories. Therefore, our intra-industry interlocks should not be interpreted as competitor interlocks (Zajac 1988).

¹⁷ In untabulated results, we further separate $L-T\ TIES_{sim}$ into high-to-high ties and low-to-low ties, and we find no significant difference in the coefficients on these two types of similar ties.

Panels A and B of Table 4 report the results of testing H2. In Panel A, we examine the effect of both intra- and extra-industry *low-tax* ties. The coefficient on *L-T TIES IND_{sim}* is not significant ($p = 0.15$), while the coefficient on *L-T TIES IND_{dissim}* is negative and marginally significant ($p = 0.07$).¹⁸ As the F-test reported at the bottom of Panel A shows, the coefficient on *L-T TIES IND_{sim}* is not significantly different from the coefficient on *L-T TIES IND_{dissim}*. The results from comparing intra- and extra-industry ties are not consistent with H2, and given there is no statistical difference in the effect of similar versus dissimilar ties when network partner similarity is based on industry, we focus on other measures of similarity that may be more germane to our setting.

The first column of Table 4, Panel B reports the results of testing H2 when we measure similarity based on income mobility. The coefficient on *L-T TIES IncomeMobile_{sim}* is negative and significant ($p = 0.02$), and the coefficient on *L-T TIES IncomeMobile_{dissim}* is not significant. Consistent with H2, an F-test reported at the bottom of Panel B indicates that the coefficient on similar income mobile ties is marginally significantly more negative than the coefficient on dissimilar ties ($p = 0.06$). We find similar results when we measure network partner similarity based on either growth opportunities or risk. The coefficient on *L-T TIES Growth_{sim}* is negative and significant ($p < 0.01$), as is the coefficient on *L-T TIES Risk_{sim}* ($p = 0.01$), but dissimilar ties are not significantly associated with *CETR3*. F-tests reported at the bottom of Panel B show that the coefficients on similar growth/risk ties are significantly more negative than the coefficients on dissimilar ties. Overall, we interpret the results of Table 4 as generally providing support for H2. Ties to operationally and strategically similar network partners appear to have more influence on tax avoidance at the focal firm compared to dissimilar ties.

Ties Involving Executive Directors

H3 predicts that the association between a firm's tax avoidance and its *low-tax* ties will be stronger when those *low-tax* ties are formed by executive directors relative to non-executive directors. To test H3, we partition *L-T TIES* into *L-T TIES_{EXEC}* and *L-T TIES_{NONEXEC}*. We designate *L-T TIES* as *L-T TIES_{EXEC}* where the shared director is the CEO or CFO of either one of the interlocked firms. We expect ties formed by executive directors to be influential; therefore, we expect the coefficient on *L-T TIES_{EXEC}* to be negative and significant. We make no specific prediction about the influence of non-executive directors; however, if non-executive directors are influential, then we expect those ties to be negatively related to *CETR3*. Specifically, H3 implies that the coefficient on *L-T TIES_{EXEC}* will be greater (more negative) than the coefficient on *L-T TIES_{NONEXEC}*.

Table 5 presents the results of regressing tax avoidance on both *L-T TIES_{EXEC}* and *L-T TIES_{NONEXEC}* together. We find that the coefficient on *L-T TIES_{EXEC}* is negative and significant ($p < 0.01$), while the coefficient on *L-T TIES_{NONEXEC}* is not significant. The influence of *low-tax* ties appears to be attributable to *low-tax* ties formed by executive directors rather than non-executive directors.¹⁹ In support of H3, when we compare the coefficients on *L-T TIES_{EXEC}* and *L-T TIES_{NONEXEC}*, we find that the coefficient on *L-T TIES_{EXEC}* is marginally more negative ($p = 0.06$).

¹⁸ Note that our findings relate to intra-industry and extra-industry *board ties* and therefore differ from those in Brown (2011). She finds no evidence that the aggregate number of COLI adopters in the focal firm's industry increases the likelihood of COLI adoption at the focal firm; however, she does not examine the contingent effect of *board ties* to firms within the focal firm's industry.

¹⁹ In untabulated analysis, we distinguish interlocks where the focal firm sends an executive to another board from interlocks where the focal firm receives an executive from another board. However, we find no significant difference between the effect of sending or receiving an executive board member. Although this finding does not offer any additional insights into the nuances of interlock relationships, it is consistent with prior studies that find no differential effect for sent versus received ties (Beckman and Haunschild 2002; Haunschild and Beckman 1998).

TABLE 4
OLS Regression of Tax Avoidance (CETR3) on a Firm's Ties to Similar and Dissimilar Low-tax Firms
(Network Ties to Similar Versus Dissimilar Firms Based on Industry, IncomeMobile Status, Growth, and Risk)

$$CETR3_{i,p} = \beta_0 + \beta_1 L-T\ TIES\ XXX_{sim\ i,p} + \beta_2 L-T\ TIES\ XXX_{neutral\ i,p} + \beta_3 L-T\ TIES\ XXX_{dissim\ i,p} + \beta_4 SIZE3_{i,p} + \beta_5 ROA3_{i,p} + \beta_6 LEV3_{i,p} + \beta_7 PPE3_{i,p} + \beta_8 INTAN3_{i,p} + \beta_9 RD3_{i,p} + \beta_{10} AD3_{i,p} + \beta_{11} NOL3_{i,p} + \beta_{12} FOR3_{i,p} + \beta_{13} SPI3_{i,p} + industry\ controls + period\ controls + \varepsilon.$$

Panel A: Similar/Dissimilar Industry Low-Tax TIES

	Predicted Sign	Similar/Dissimilar Industry Low-Tax TIES	
		Coeff.	t-stat
L-T TIES IND _{sim}	—	−0.016	−1.44
L-T TIES IND _{dissim}	—	−0.015	−1.79*
SIZE3	?	−0.005	−2.63***
ROA3	?	−0.169	−3.75***
LEV3	?	−0.137	−5.28***
PPE3	—	−0.007	−0.81
INTAN3	—	0.039	1.34
RD3	—	−0.014	−2.39**
AD3	+	−0.015	−0.24
NOL3	—	0.008	0.25
FOR3	—	−0.045	−0.45
SPI3	+	0.317	2.02**
Intercept		Yes	
R ²		7.20%	
F-test:			
$\beta_{L-T\ TIES\ IND_{sim}} - \beta_{L-T\ TIES\ IND_{dissim}} = 0$			
F = 0.01 (p = 0.92)			

Panel B: Similar/Dissimilar IncomeMobile, Growth, Risk Low-Tax TIES

	Predicted Sign	Similar/Dissimilar IncomeMobile Low-Tax TIES		Similar/Dissimilar Growth Low-Tax TIES		Similar/Dissimilar Risk Low-Tax TIES	
		Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
L-T TIES IncomeMobile _{sim}	—	−0.038	−2.30**				
L-T TIES IncomeMobile _{neutral}	—	−0.002	−0.19				
L-T TIES IncomeMobile _{dissim}	—	0.004	0.11				
L-T TIES Growth _{sim}	—			−0.058	−3.97***		
L-T TIES Growth _{neutral}	—			−0.011	−1.15		
L-T TIES Growth _{dissim}	—			0.015	0.67		
L-T TIES Risk _{sim}	—					−0.020	−2.57**

(continued on next page)

TABLE 4 (continued)

	Predicted Sign	Similar/Dissimilar <i>IncomeMobile</i> <i>Low-Tax TIES</i>		Similar/Dissimilar <i>Growth</i> <i>Low-Tax TIES</i>		Similar/Dissimilar <i>Risk Low-Tax TIES</i>	
		Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
<i>L-T TIES Risk_{neutral}</i>	—					−0.015	−1.26
<i>L-T TIES Risk_{dissim}</i>	—					0.023	1.20
<i>SIZE3</i>	?	−0.005	−2.72***	−0.005	−2.71***	−0.005	−2.60***
<i>ROA3</i>	?	−0.168	−3.75***	−0.173	−3.79***	−0.169	−3.77***
<i>LEV3</i>	?	−0.137	−5.28***	−0.137	−5.28***	−0.137	−5.26***
<i>PPE3</i>	—	−0.007	−0.84	−0.007	−0.85	−0.007	−0.79
<i>INTAN3</i>	—	0.039	1.35	0.038	1.31	0.039	1.36
<i>RD3</i>	—	−0.014	−2.37**	−0.015	−2.45**	−0.014	−2.40**
<i>AD3</i>	+	−0.011	−0.17	−0.014	−0.22	−0.014	−0.22
<i>NOL3</i>	—	0.008	0.24	0.007	0.22	0.006	0.19
<i>FOR3</i>	—	−0.039	−0.38	−0.040	−0.40	−0.045	−0.45
<i>SPI3</i>	+	0.320	2.03**	0.316	2.01**	0.317	2.02**
Intercept		Yes		Yes		Yes	
R ²		7.20%		7.30%		7.20%	

F-tests:

$\beta L-T\ TIES\ IncomeMobile_{sim} < \beta L-T\ TIES\ IncomeMobile_{dissim}$ F = 2.40 * (p = 0.06)
 $\beta L-T\ TIES\ Growth_{sim} < \beta L-T\ TIES\ Growth_{dissim}$ F = 7.64*** (p = 0.00)
 $\beta L-T\ TIES\ Risk_{sim} < \beta L-T\ TIES\ Risk_{dissim}$ F = 5.07** (p = 0.01)

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.
n = 13,784.
All regressions include industry and period controls, and standard errors are clustered by firm and by period. All variables except *CETR3* and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3*, and observations with *CETR3* greater than 1 are winsorized at 1.
See Appendix A for complete variable definitions.

Ties Involving Shared Local Auditors

H4 posits that shared local auditors lend credence to the information communicated via board ties, increasing the influence of those ties on tax avoidance at the focal firm. To test H4, we separate *L-T TIES* into ties to *low-tax* firms audited by the focal firm’s local auditor (*L-T TIES local AUDITOR_{same}*) and ties to *low-tax* firms that are not audited by the focal firm’s auditor (*L-T TIES local AUDITOR_{diff}*),²⁰ where local is defined by metropolitan statistical area, MSA. We expect the coefficient on *L-T TIES local AUDITOR_{same}* to be negative and to be greater (more negative) than the coefficient on *L-T TIES local AUDITOR_{diff}*.

Table 6 presents the results for our tests of H4. When we separate *low-tax* ties in our regression model into same-local-auditor *low-tax* ties and different-auditor *low-tax* ties, we find that the coefficient on *L-T TIES local AUDITOR_{same}* is negative and significant (p < 0.01), while the coefficient on *L-T TIES local AUDITOR_{diff}* is marginally significant (p = 0.08). In support of H4,

²⁰ If a firm engages the same auditor but in a different MSA, then we classify that as *local AUDITOR_{diff}*. Likewise, if the firm engages a different auditor in the same MSA, then we classify that as *local AUDITOR_{diff}*.

TABLE 5
OLS Regression of Tax Avoidance (*CETR3*) on a Firm's Ties to *Low-Tax* Firms via Executive and Non-Executive Ties

$$CETR3_{i,p} = \beta_0 + \beta_1 L-T\ TIES_{EXEC\ i,p} + \beta_2 L-T\ TIES_{NONEXEC\ i,p} + \beta_3 SIZE3_{i,p} + \beta_4 ROA3_{i,p} + \beta_5 LEV3_{i,p} + \beta_6 PPE3_{i,p} + \beta_7 INTAN3_{i,p} + \beta_8 RD3_{i,p} + \beta_9 AD3_{i,p} + \beta_{10} NOL3_{i,p} + \beta_{11} FOR3_{i,p} + \beta_{12} SPI3_{i,p} + industry\ controls + period\ controls + \varepsilon.$$

	Predicted Sign	Low-Tax EXEC and NON-EXEC TIES	
		Coeff.	t-stat
<i>L-T TIES_{EXEC}</i>	—	−0.028	−2.82***
<i>L-T TIES_{NONEXEC}</i>	—	−0.009	−0.92
<i>SIZE3</i>	?	−0.005	−2.61***
<i>ROA3</i>	?	−0.169	−3.75***
<i>LEV3</i>	?	−0.137	−5.28***
<i>PPE3</i>	—	−0.007	−0.80
<i>INTAN3</i>	—	0.039	1.35
<i>RD3</i>	—	−0.014	−2.41**
<i>AD3</i>	+	−0.016	−0.24
<i>NOL3</i>	—	0.009	0.28
<i>FOR3</i>	—	−0.045	−0.45
<i>SPI3</i>	+	0.317	2.03**
Intercept		Yes	
R ²		7.22%	

F-test:
 $\beta_{L-T\ TIES_{EXEC}} < \beta_{L-T\ TIES_{NONEXEC}}$ F = 2.41* (p = 0.06)

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.
n = 13,784
All regressions include industry and period controls, and standard errors are clustered by firm and by period.
All variables except *CETR3* and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3*, and observations with *CETR3* greater than 1 are winsorized at 1.
See Appendix A for complete variable definitions.

we find that the coefficient on *L-T TIES local AUDITOR_{same}* is greater (more negative) than the coefficient on *L-T TIES local AUDITOR_{diff}* (p = 0.02).²¹

V. ADDITIONAL ANALYSIS AND DISCUSSION

The Effect of a Change in Network Ties on Tax Avoidance

Our study is motivated in part by Dyreng et al. (2008), who identify a set of firms that are able to achieve low cash ETRs over a decade. We expect that these long-term tax avoiders are

²¹ In untabulated tests we examine whether tax avoidance at the focal firm is influenced by direct local auditor ties to *low-tax* firms (without a board interlock), but do not find a significant association between *CETR3* and the number of *low-tax* firms that also engage the focal firm's same local auditor. Our results are consistent with Brown (2011) and Reppenhagen (2010), who do not find any statistically significant evidence of an auditor tie effect. Our findings for H4 are also robust to including direct local auditor ties as an additional control. Similarly, we find no evidence of a geographic proximity effect; the number of *low-tax* firms in a focal firm's MSA is not associated with a firm's tax-avoidance behavior. All of our results are robust to including auditor fixed effects.

TABLE 6

OLS Regression of Tax Avoidance (CETR3) on a Firm’s Ties to Low-Tax Firms via Shared Local Auditor Ties

$$CETR3_{i,p} = \beta_0 + \beta_1 L-TTIES\ local\ AUDITOR_{same\ i,p} + \beta_2 L-TTIES\ local\ AUDITOR_{diff\ i,p} + \beta_3 SIZE3_{i,p} + \beta_4 ROA3_{i,p} + \beta_5 LEV3_{i,p} + \beta_6 PPE3_{i,p} + \beta_7 INTAN3_{i,p} + \beta_8 RD3_{i,p} + \beta_9 AD3_{i,p} + \beta_{10} NOL3_{i,p} + \beta_{11} FOR3_{i,p} + \beta_{12} SPI3_{i,p} + industry\ controls + period\ controls + \varepsilon.$$

	Predicted Sign	Same and Different Auditor Low-Tax TIES	
		Coeff.	t-stat
L-T TIES local AUDITOR _{same}	—	−0.056	−3.02***
L-T TIES local AUDITOR _{diff}	—	−0.014	−1.74*
SIZE3	?	−0.005	−2.73***
ROA3	?	−0.159	−3.49***
LEV3	?	−0.136	−4.97***
PPE3	—	−0.007	−0.77
INTAN3	—	0.037	1.30
RD3	—	−0.014	−2.35**
AD3	+	−0.009	−0.13
NOL3	—	−0.008	−0.20
FOR3	—	−0.056	−0.55
SPI3	+	0.304	1.77*
Intercept		Yes	
R ²		7.50%	

F-test:

$\beta L-T\ TIES\ local\ AUDITOR_{same} < \beta L-T\ TIES\ local\ AUDITOR_{diff}$ F = 4.05** (p = 0.02)

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.

n = 13,784.

All regressions include industry and period controls, and standard errors are clustered by firm and by period.

All variables except CETR3 and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative CETR3, and observations with CETR3 greater than 1 are winsorized at 1.

See Appendix A for complete variable definitions.

privity to valuable information about and experience with tax-planning strategies, and to provide further evidence regarding the influence of network ties on tax avoidance, we examine the change in tax avoidance that occurs when a firm becomes connected to one of these long-term tax avoiders.

First, we identify long-term tax avoiders within our sample as those firms ranked in the lowest quintile based on CETR10_IND, industry-adjusted, cash ETR calculated over the ten-year period, 1996–2006. We label these long-term tax avoiders as L-T10 firms. Next, we find the set of non-L-T10 firms that start our 12-year sample timeframe with no ties to L-T10 firms, but become connected to an L-T10 firm during the sample timeframe. We match these firms, based on period, size, and lag CETR3, to a control sample of firms that are never tied to an L-T10 firm and estimate the following regression:

$$\begin{aligned} \Delta CETR3_i = & \beta_0 + \beta_1 \Delta L-T10\ TIES_i + \beta_2 CETR3_{i,p-1} + \beta_3 \Delta SIZE3_i + \beta_4 \Delta ROA3_i + \beta_5 \Delta LEV3_i \\ & + \beta_6 \Delta PPE3_i + \beta_7 \Delta INTAN3_i + \beta_8 \Delta RD3_i + \beta_9 \Delta AD3_i + \beta_{10} \Delta FOR3_i \\ & + \beta_{11} \Delta SPI3_i + industry\ controls + period\ controls + \varepsilon. \end{aligned} \tag{2}$$

The dependent variable in Equation (2) is the change in a firm’s three-year cash ETR (*CETR3*) from period *p*–1 to period *p*, where *p*–1 indicates the three-year period ending the year prior to the first year of period *p*.²² Likewise, all other variables are calculated as changes from period *p*–1 to period *p*. We also control for the tax avoidance of the firm in period *p*–1 by including *CETR3*, recognizing that a firm with a higher level of cash ETR can more easily reduce its level of cash ETR.

Table 7 presents the results from estimating the change model in Equation (2). The coefficient on *ΔL-T10 TIES* is negative and significant (*p* < 0.01), indicating that establishing a tie to a long-term tax avoider is associated with an *increase* in the focal firm’s tax-avoidance behavior. Based on our regression estimates, firms that become connected to long-term tax avoiders experience an average decrease in their cash ETRs of 1.1 percent. Given mean annual pretax book income of \$568.11 million, this 1.1 percent decrease translates to \$6.25 million in cash tax savings.

Alternative Explanations

Because we treat board interlocks as exogenous, our study, like other studies about social networks and performance, is subject to endogeneity concerns (Brass et al. 2004). Differences in firm performance might influence network partner selection. Another possibility is that board connections provide a conduit for the diffusion of broader firm strategies and operating practices, and low tax rates are simply a consequence of the focal firm implementing these broader strategies and practices. If these alternative explanations are true, then our findings do not really reflect shared knowledge or experience related to tax-savings strategies. We perform several tests to assess whether these alternative explanations are driving our main results and report the results of these tests in Table 8.

Endogenous director-firm matching suggests that firms seek to establish ties with directors who likely have experience at better-performing firms and *vice versa*. To assess the impact of this process on our results, we examine whether ties to firms with superior market, accounting, and operating metrics are associated with tax avoidance at the focal firm. We find no significant association between ties to firms with high operating margins (*High-GP ties*) or those with high stock return performance (*High-Return ties*) and *CETR3*, and we find that ties to highly profitable firms (*High-ROA ties*) are positively related to cash ETR at the focal firm. Thus, although firms may seek to establish network ties with their well-managed peers and these ties may influence practices at the focal firm, it is unlikely these connections are driving our results. In untabulated analysis, we repeat the regressions presented in Table 8 and include *L-T TIES* in each specification. We find that the coefficient on *L-T TIES* remains negative and significant, indicating that low-tax ties continue to explain tax avoidance at the focal firm.

If connected firms share broader strategies and operating practices, and similar tax rates are merely a consequence of this process, then the network effect should be symmetric and firms with *high-tax* ties should exhibit less tax avoidance (i.e., have higher cash ETRs).²³ However, we do not

²² Equation (2) does not include a control for net operating losses because *ΔNOL* equals 0 for all firms in the match-control sample.

²³ We thank our anonymous reviewer for bringing this to our attention.

TABLE 7

OLS Regression of the Change in Tax Avoidance on the Change in a Firm’s Ties to Long-Term Low-Tax Firms

$$\Delta CETR3_i = \beta_0 + \beta_1 \Delta L-T10\ TIES_i + \beta_2 CETR3_{i,p-1} + \beta_3 \Delta SIZE3_i + \beta_4 \Delta ROA3_i + \beta_5 \Delta LEV3_i + \beta_6 \Delta PPE3_i + \beta_7 \Delta INTAN3_i + \beta_8 \Delta RD3_i + \beta_9 \Delta AD3_i + \beta_{10} \Delta FOR3_i + \beta_{11} \Delta SPI3_i + industry\ controls + period\ controls + \varepsilon.$$

1-1 Match Sample	Predicted Sign	Coeff.	t-stat
$\Delta L-T10\ TIES$	—	−0.081	−2.82***
$CETR3_{p-1}$	?	−0.731	−8.48***
$\Delta SIZE3$	?	−0.078	−1.81*
$\Delta ROA3$	?	−0.168	−0.55
$\Delta LEV3$	?	0.085	0.94
$\Delta PPE3$	—	−0.039	−0.39
$\Delta INTAN3$	—	−0.067	−0.75
$\Delta RD3$	—	−0.549	−1.60
$\Delta AD3$	+	0.018	0.27
$\Delta FOR3$	—	0.901	1.50
$\Delta SPI3$	+	0.003	1.09
Intercept		Yes	
R ²		36.70%	

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.
n = 208.
The Δ prefix indicates the change in the variable from period p−1 to period p, where p−1 indicates the three-year period ending the year prior to the first year of period p.
All regressions include industry and period controls, and standard errors are clustered by firm and by period.
All variables except CETR3 and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative CETR3, and observations with CETR3 greater than 1 are winsorized at 1.
See Appendix A for complete variable definitions.

find this result. Firms have no incentive to mimic the strategies utilized by their high-tax network partners. Our asymmetric result, that cash ETR at the focal firm is associated with the focal firm’s low-tax ties but not its high-tax ties, is consistent with firms gaining knowledge and experience related to tax-savings strategies from their board connections.

Measuring Network Ties—Centrality

Recent work by Larcker et al. (2013) explores the relation between board centrality and firm performance and finds that firms occupying central positions in the interlocking directorate network earn superior risk-adjusted stock returns. Larcker et al.’s (2013) results are consistent with theory suggesting actors who are centrally located within the network may derive the most benefit from the network because central firms are exposed to more information overall (Gulati 1999; Granovetter 2005; Burt 1980). Following this line of reasoning, one might expect more central firms in the network to exhibit lower cash ETRs, since these firms should be exposed to the widest set of tax-avoidance strategies. However, we do not find this result.

TABLE 8
OLS Regression of Tax Avoidance (*CETR*) on a Firm's Ties to *High-Tax Firms*, *High-ROA Firms*, *High-Gross Profit Firms*, and *High-Return Firms*

	Predicted Sign	High-ROA TIES		High-GP TIES		High-Return TIES		High-Tax TIES	
		Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
High-ROA TIES	—	0.016	2.09**						
High-GP TIES	—			0.003	0.32				
High-Return TIES	—					−0.008	−1.17		
High-Tax TIES	—							0.008	1.18
SIZE3	?	−0.006	−2.94***	−0.005	−2.88***	−0.005	−2.77***	−0.005	−2.85***
ROA3	?	−0.176	−3.95***	−0.174	−3.86***	−0.174	−3.85***	−0.174	−3.85***
LEV3	?	−0.136	−5.15***	−0.136	−5.15***	−0.137	−5.18***	−0.136	−5.15**
PPE3	—	−0.007	−0.80	−0.007	−0.79	−0.007	−0.80	−0.007	−0.80
INTAN3	—	0.039	1.34	0.039	1.35	0.040	1.36	0.039	1.35
RD3	—	−0.014	−2.39**	−0.014	−2.36**	−0.014	−2.34**	−0.014	−2.34**
AD3	+	−0.017	−0.27	−0.014	−0.22	−0.014	−0.22	−0.014	−0.21
NOL3	—	0.005	0.14	0.004	0.11	0.004	0.10	0.004	0.10
FOR3	—	−0.029	−0.28	−0.029	−0.27	−0.026	−0.25	−0.029	−0.28
SPI3	+	0.314	1.99**	0.313	1.99**	0.313	2.00**	0.312	1.99**
Intercept		Yes		Yes		Yes		Yes	
R ²		7.50%		7.40%		7.40%		7.40%	

*, **, *** Indicate significance at the 0.1, 0.05, and 0.01 levels, respectively, for two-tailed tests.
n = 13,784.
All regressions include industry and period controls, and standard errors are clustered by firm and by period.
All variables except *CETR3* and indicator variables are winsorized at 1 and 99 percent. The sample excludes observations with negative *CETR3*, and observations with *CETR3* greater than 1 are winsorized at 1.
See Appendix A for complete variable definitions.

Using UCINET (Borgatti, Everett, and Freeman 2002), we construct two common measures of network centrality, *EIGENVECTOR* and *DEGREE*.²⁴ In untabulated tests, we find a weakly positive relation between these measures and cash ETR. One explanation for this contrary result is that in our sample, both *EIGENVECTOR* and *DEGREE* are highly correlated to firm size; as expected, larger firms are more connected. Our evidence does not necessarily contradict Larcker et al. (2013). Instead, our results suggest that in a particular setting, the nature and context of a firm's network ties are important, rather than centrality itself.

Measuring Tax Avoidance—GAAP ETR

We posit that interlocked directors serve as conduits for the diffusion of tax-savings strategies, and we use long-run cash ETR to proxy for a firm's tax-avoidance behavior. To the extent that managers prefer strategies that also decrease GAAP ETR, we would expect to find a similar

²⁴ *DEGREE*, which is equivalent to our measure, *TIES*, represents the number of first-degree links a firm has to other boards, and *EIGENVECTOR* captures how well connected a firm's direct contacts are. We do not explore the role of additional measures of indirect board links because we posit that information on tax strategies is tacit and complex, and the role of indirect board links in the diffusion of tacit and complex knowledge is unclear.

relationship between a firm's GAAP ETR and its connections via board interlocks to firms in the lowest quintile of industry-adjusted GAAP ETRs. In supplementary tests, we use three-year GAAP ETR as our dependent variable and construct our ties variables based on ties to firms within the bottom quintile of industry-adjusted GAAP ETR. We do not find a significant relation between GAAP ETRs and connections to *low-tax* GAAP ETR firms. Our lack of results using GAAP ETR is likely related to the lack of variance in GAAP ETR within our sample.²⁵

Limitations to Increasing Board Connections

Although there appears to be an economically significant benefit to becoming connected to a *low-tax* firm, neither establishing new board ties nor altering the firm's current board ties to garner these tax savings is costless. Frictions and constraints include limits on the number of directors the board can accommodate, limits on the number of board seats a single director can manage, and contractual or legal limitations that preclude interlocks between competitors or customers and suppliers (Larcker et al. 2013). In untabulated analysis, we test for a difference between having only one *low-tax* tie and having more than one *low-tax* tie. We find that having one *low-tax* tie is negatively associated with *CETR3* at the focal firm, as is having more than one *low-tax* tie. However, we do not find a statistical difference between the two effects.²⁶ As our study indicates, different types of ties are beneficial in different contexts; thus, orchestrating the firm's network structure to make gains in one area may not lead to an overall net advantage.

Other Types of Social Connections

While our study focuses on board interlocks, we acknowledge that a variety of other social network ties could influence corporate tax-avoidance behavior. These ties can arise directly at the organizational level, for example from membership in trade organizations or through participation in joint ventures, or indirectly via the interpersonal connections of the board members (Carpenter et al. 2012). Examples of the latter include memberships in exclusive social clubs, shared educational alumni affiliations, and appointments to charity boards (see Cohen, Frazzini, and Malloy 2008; Engelberg, Gao, and Parsons 2012). While numerous ties could potentially influence corporate tax avoidance, the board interlock network has been viewed as an ideal setting for examining the influence of social ties on firm behaviors because it is a unique formal mechanism that links together corporate decision-makers (Gulati and Westphal 1999). Board ties allow for particularly vivid learning because directors have an opportunity to observe both strategic decisions and the consequences of those decisions first-hand in their advising and monitoring roles (Carpenter and Westphal 2001; Haunschild 1993; Hillman and Dalziel 2003).

Few studies consider an actor's multiple networks, and those that do implicitly treat the influence of each as independent (Zaheer, Gözübüyük, and Milanov 2010).²⁷ Our study considers the interplay between board ties and auditor ties, but does not cross levels of analysis to consider how additional social networks possessed by individual directors might impact corporate tax avoidance. Two challenges for future research are to investigate how interpersonal ties map into interorganizational network phenomena and to determine how possible complementarities or

²⁵ While the standard deviations of our measures of cash ETR and GAAP ETR are similar, because the mean of GAAP ETR is higher than cash ETR, the true variation of GAAP ETR is smaller than cash ETR.

²⁶ We do not expect the influence of *low-tax* ties to be necessarily linear. However, given our data constraints, investigating diminishing returns to network ties remains an area for future research.

²⁷ An exception is Mizruchi (1989) who considers whether the positive effect of indirect interlocks on the similarity of political behavior is contingent on the presence of a direct interlock.

redundancies arising from multiple contemporaneous networks influence firm-level behavior and outcomes (Zaheer et al. 2010; Brass et al. 2004).

VI. CONCLUSION

This paper investigates corporate tax avoidance by examining the social structure in which tax-avoidance behavior is embedded. We find that firms with greater *low-tax* network ties exhibit greater tax avoidance themselves. Once we establish a basic network effect for tax avoidance, we draw on theory from management and sociology to formulate hypotheses about the influence of network ties across three settings: (1) ties to similar/dissimilar network partners, (2) ties formed by executive/non-executive directors, and (3) ties to network partners that share a common local auditor. We find that tax avoidance is associated with a firm's *low-tax* ties to similar network partners suggesting that, in the transfer of tax knowledge, network partner similarity may be particularly beneficial. We also find that ties created by executive directors are more influential than ties created by non-executive directors. Finally, we find that board ties have more influence on tax avoidance when network partners share a common local auditor.

Our study directly responds to the call in Hanlon and Heitzman (2010) for more research on the social connections between tax-avoiding firms and makes a unique contribution to the literature on tax avoidance. Dyreng et al. (2008) identify a set of firms that are able to maintain low cash ETRs and present preliminary descriptives of those firms. We provide evidence that network ties explain part of the observed variation in corporate tax avoidance.

Our study also contributes to the literatures on social networks and director interlocks by providing a more nuanced understanding of the impact that board ties can have on firm performance. Much of the research on social networks focuses on the benefit of having weak ties or an overall central position in the network (Burt 1992; Granovetter 1973; Larcker et al. 2013). However, our results demonstrate that the effects associated with board ties depend on the nature of the network tie and the information being shared across that tie. In particular, our finding that similar ties influence tax avoidance at the focal firm is consistent with Hansen's (1999) theory that strong ties rather than weak ties facilitate the transfer of tacit and complex information. Although our results relate to one type of firm behavior, tax avoidance, the concepts and theories we introduce in this study should apply and be of interest in future studies of social networks and a broad range of accounting-related behaviors (e.g., earnings quality, managerial compensation, internal control weaknesses).

Last, our study is related to recent work in accounting that examines whether executives have idiosyncratic, individual-level effects on corporate decisions and outcomes (Bamber, Jiang, and Wang 2010; Dyreng et al. 2010; Ge, Matsumoto, and Zhang 2011). These studies stem from Hambrick and Mason's (1984) influential upper-echelons theory that also predicts that managerial characteristics, as proxies for differences in managers' cognitions, values, and perceptions, will affect strategic choices and firm outcomes. Dyreng et al. (2010) find evidence of a manager fixed-effect for tax avoidance, but find little connection between this fixed-effect and executive biographical characteristics.

Geletkanycz and Hambrick (1997) connect theory on upper echelons together with theory on social networks. While empirical research on firm behavior and upper echelons has tended to focus on executives' backgrounds and personal characteristics, Geletkanycz and Hambrick (1997) show that executives influence firm behavior via their network ties to external organizations. We find that *low-tax* ties involving executive directors are associated with lower cash ETRs at the focal firm. Our findings complement those presented in Dyreng et al. (2010) and suggest that executives in their role as directors have an impact on tax avoidance that extends beyond their home firms.

REFERENCES

- Adler, P. S., and S. W. Kwon. 2002. Social capital: Prospects for a new concept. *Academy of Management Review* 27 (1): 17–40.
- Argote, L., B. McEvily, and R. Reagans. 2003. Managing knowledge in organizations: An integrative framework and review of emerging themes. *Management Science* 49 (4): 571–582.
- Armstrong, C., J. Blouin, and D. Larcker. 2012. The incentives for tax planning. *Journal of Accounting and Economics* 53 (1-2): 391–411.
- Bamber, L. S., J. X. Jiang, and I. Y. Wang. 2010. What's my style? The influence of top managers and their personal backgrounds on voluntary corporate financial disclosure. *The Accounting Review* 85 (4): 1131–1162.
- Barth, M., W. Beaver, and W. Landsman. 1998. Relative valuation roles of equity book value and net income as a function of financial health. *Journal of Accounting and Economics* 25 (1): 1–34.
- Beckman, C. M., and P. R. Haunschild. 2002. Network learning: The effects of partners' heterogeneity of experience on corporate acquisitions. *Administrative Science Quarterly* 47 (1): 92–124.
- Bizjak, J., M. Lemmon, and R. Whitby. 2009. Option backdating and board interlocks. *Review of Financial Studies* 22 (11): 4821–4847.
- Bonacich, P. 1972. Technique for analyzing overlapping memberships. *Sociological Methodology* 4: 176–185.
- Borgatti, S. P., M. G. Everett, and L. C. Freeman. 2002. *UCINET 6 for Windows: Software for Social Network Analysis*. Harvard, MA: Analytic Technologies.
- Borgatti, S. P., and R. Cross. 2003. A relational view of information seeking and learning in social networks. *Management Science* 49 (4): 432–445.
- Borgatti, S. P., and P. C. Foster. 2003. The network paradigm in organizational research: A review and typology. *Journal of Management* 29 (6): 991–1013.
- Brass, D. J., K. D. Butterfield, and B. C. Skaggs. 1998. Relationships and unethical behavior: A social network perspective. *Academy of Management Review* 23 (1): 14–31.
- Brass, D. J., J. Galaskiewicz, H. R. Greve, and W. Tsai. 2004. Taking stock of networks and organizations: A multilevel perspective. *Academy of Management Journal* 47 (6): 795–817.
- Brown, J. L. 2011. The spread of aggressive corporate tax reporting: A detailed examination of the corporate-owned life insurance shelter. *The Accounting Review* 86 (1): 23–57.
- Burt, R. S. 1980. Cooptive corporate actor networks: A reconsideration of interlocking directorates involving American manufacturing. *Administrative Science Quarterly* 25 (4): 557–582.
- Burt, R. S. 1992. *Structural Holes: The Social Structure of Competition*. Cambridge, MA: Harvard University Press.
- Carpenter, M. A., and J. D. Westphal. 2001. The strategic context of external network ties: Examining the impact of director appointments on board involvement in strategic decision making. *Academy of Management Journal* 44 (4): 639–660.
- Carpenter, M. A., M. Li, and H. Jiang. 2012. Social network research in organizational contexts: A systematic review of methodological issues and choices. *Journal of Management* 38 (4): 1328–1361.
- Chen, S., X. Chen, Q. Cheng, and T. Shevlin. 2010. Are family firms more tax aggressive than non-family firms? *Journal of Financial Economics* 95 (1): 41–61.
- Chiu, P.-C., S. H. Teoh, and F. Tian. 2013. Board interlocks and earnings management contagion. *The Accounting Review* 88 (3): 915–944.
- Cohen, L., A. Frazzini, and C. Malloy. 2008. The small world of investing: Board connections and mutual fund returns. *Journal of Political Economy* 116 (5): 951–979.
- Davis, G. F. 1991. Agents without principles? The spread of the poison pill through the intercorporate network. *Administrative Science Quarterly* 36 (4): 583–613.
- Davis, G. F., and H. R. Greve. 1997. Corporate elite networks and governance changes in the 1980s. *American Journal of Sociology* 103: 1–37.
- De Simone, L., and B. Stomberg. 2012. *Do Investors Differentially Value Tax Avoidance of Income Mobile Firms?* Working paper, The University of Texas.

- Desai, M., and D. Dharmapala. 2006. Corporate tax avoidance and high-powered incentives. *Journal of Financial Economics* 79 (1): 145–179.
- Dhaliwal, D., G. Lee, M. Pincus, and L. Steele. 2012. *Taxable Income Properties, the Predictability of Book Income, and Firm Risk*. Working paper, The University of Arizona.
- Drake, K. 2013. *Does Firm Life Cycle Explain the Relation Between Book-Tax Differences and Earnings Persistence?* Working paper, The University of Arizona.
- Dyreng, S., M. Hanlon, and E. L. Maydew. 2008. Long-run corporate tax avoidance. *The Accounting Review* 83 (1): 61–82.
- Dyreng, S., M. Hanlon, and E. L. Maydew. 2010. The effects of executives on corporate tax avoidance. *The Accounting Review* 85 (4): 1163–1189.
- Dyreng, S. D., and B. P. Lindsey. 2009. Using financial accounting data to examine the effect of foreign operations located in tax havens and other countries on U.S. multinational firms' tax rates. *Journal of Accounting Research* 47 (5): 1283–1316.
- Engelberg, J., P. Gao, and C. A. Parsons. 2012. Friends with money. *Journal of Financial Economics* 103: 169–188.
- Friedkin, N. E. 1991. Theoretical foundations for centrality measures. *American Journal of Sociology* 96 (6): 1478–1504.
- Ge, W., D. Matsumoto, and J. L. Zhang. 2011. Do CFOs have style? An empirical investigation of the effect of individual CFOs on accounting practices. *Contemporary Accounting Research* 28 (4): 1141–1179.
- Geletkanycz, M. A., and D. C. Hambrick. 1997. The external ties of top executives: Implications for strategic choice and performance. *Administrative Science Quarterly* 42 (4): 654–681.
- Granovetter, M. 1973. The strength of weak ties. *American Journal of Sociology* 78: 1360–1380.
- Granovetter, M. 2005. The impact of social structure on economic outcomes. *The Journal of Economic Perspectives* 19 (1): 33–50.
- Gulati, R., and J. D. Westphal. 1999. Cooperative or controlling? The effects of CEO-board relations and the content of interlocks on the formation of joint ventures. *Administrative Science Quarterly* 44 (3): 473–506.
- Gulati, R. 1999. Network location and learning: The influence of network resources and firm capabilities on alliance formation. *Strategic Management Journal* 20 (5): 397–420.
- Gupta, S., and K. Newberry. 1997. Determinants of the variability in corporate effective tax rates: Evidence from longitudinal data. *Journal of Accounting and Public Policy* 16 (1): 1–34.
- Hambrick, D. C., and P. A. Mason. 1984. Upper echelons: The organization as a reflection of its top managers. *Academy of Management Review* 9 (2): 193–206.
- Hanlon, M., L. Mills, and J. Slemrod. 2005. An empirical examination of corporate tax noncompliance. In *Taxing Corporate Income in the 21st Century*. Cambridge, MA: Cambridge University Press.
- Hanlon, M., and J. Slemrod. 2009. What does tax aggressiveness signal? Evidence from stock price reactions to news about tax shelter involvement. *Journal of Public Economics* 93 (1-2): 126–141.
- Hanlon, M., and S. Heitzman. 2010. A review of tax research. *Journal of Accounting and Economics* 50 (2-3): 127–178.
- Hansen, M. T. 1999. The search-transfer problem: The role of weak ties in sharing knowledge across organization subunits. *Administrative Science Quarterly* 44 (1): 82–111.
- Haunschild, P. R. 1993. Interorganizational imitation: The impact of interlocks on corporate acquisition activity. *Administrative Science Quarterly* 38 (4): 564–592.
- Haunschild, P. R., and C. M. Beckman. 1998. When do interlocks matter? Alternate sources of information and interlock influence. *Administrative Science Quarterly* 43 (4): 815–844.
- Hillman, A. J., and T. Dalziel. 2003. Boards of directors and firm performance: Integrating agency and resource dependence perspectives. *The Academy of Management Review* 28 (3): 383–396.
- Kim, J. B., Y. Li, and L. Zhang. 2011. Corporate tax avoidance and stock price crash risk: Firm-level analysis. *Journal of Financial Economics* 100 (3): 639–662.
- Larcker, D., E. So, and C. Wang. 2013. Boardroom centrality and firm performance. *Journal of Accounting and Economics* 55:225–250.

- Leenders, R. T. A. J. 2002. Modeling social influence through network autocorrelation: Constructing the weight matrix. *Social Networks* 24 (1): 21–47.
- Levin, D. Z., and R. Cross. 2004. The strength of weak ties you can trust: The mediating role of trust in effective knowledge transfer. *Management Science* 50 (11): 1477–1490.
- Lisowsky, P. 2010. Seeking shelter: Empirically modeling tax shelters using financial statement information. *The Accounting Review* 85 (5): 1693–1720.
- McDonald, M. L., J. D. Westphal, and M. E. Graebner. 2008. What do they know? The effects of outside director acquisition experience on firm acquisition performance. *Strategic Management Journal* 29 (11): 1155–1177.
- McGuire, S., T. Omer, and D. Wang. 2012. Tax avoidance: Does tax-specific industry expertise make a difference? *The Accounting Review* 87 (3): 975–1003.
- Mizruchi, M. S. 1989. Similarity of political behavior among large American corporations. *American Journal of Sociology*: 401–424.
- Mizruchi, M. S. 1996. What do interlocks do? An analysis, critique, and assessment of research on interlocking directorates. *Annual Review of Sociology*: 271–298.
- Mizruchi, M. S., and C. Marquis. 2006. Egocentric, sociocentric, or dyadic? Identifying the appropriate level of analysis in the study of organizational networks. *Social Networks* 28 (3): 187–208.
- Novack, J., and L. Saunders. 1998. The hustling of x-rated shelters. *Forbes* (December 14): 198.
- O'Neil, H. M., R. W. Pouders, and A. K. Buchholtz. 1998. Patterns in the diffusions of strategies across organizations: Insights from the innovation diffusion literature. *The Academy of Management Review* 23 (1): 98–114.
- Palmer, D., B. M. Barber, X. Zhou, and Y. Soysal. 1995. The friendly and predatory acquisition of large US corporations in the 1960s: The other contested terrain. *American Sociological Review*: 469–499.
- Palmer, D. A., P. D. Jennings, and X. Zhou. 1993. Late adoption of the multidivisional form by large US corporations: Institutional, political, and economic accounts. *Administrative Science Quarterly*: 100–131.
- Reagans, R., and B. McEvily. 2003. Network structure and knowledge transfer: The effects of cohesion and range. *Administrative Science Quarterly* 48 (2): 240–267.
- Rego, S., and R. Wilson. 2012. Equity risk incentives and corporate tax aggressiveness. *Journal of Accounting Research* 50 (3): 775–810.
- Rego, S. O. 2003. Tax-avoidance activities of US multinational corporations. *Contemporary Accounting Research* 20 (4): 805–833.
- Reppenhagen, D. A. 2010. Contagion of accounting methods: Evidence from stock option expensing. *Review of Accounting Studies* 15: 629–657.
- Rindfleisch, A., and C. Moorman. 2001. The acquisition and utilization of information in new product alliances: A strength-of-ties perspective. *The Journal of Marketing* 65 (2): 1–18.
- Robinson, J., S. Sikes, and C. Weaver. 2010. Performance measurement of corporate tax departments. *The Accounting Review* 85: 1035.
- Rogers, E. M. 2003. *Diffusion of Innovations*. 5th edition. New York, NY: Free Press.
- Shulman, D. 2009. *Prepared Remarks of IRS Commissioner Douglas Shulman before the National Association of Corporate Directors Governance Conference, Washington, DC*. <http://www.irs.gov/uac/IRS-Commissioner-Doug-Shulman-addressed-the-National-Association-of-Corporate-Directors-Governance-Conference>
- Strang, D., and S. A. Soule. 1998. Diffusion in organizations and social movements: From hybrid corn to poison pills. *Annual Review of Sociology* 24 (1): 265–290.
- Westphal, J. D., R. Gulati, and R. M. Shotell. 1997. Customization or conformity? An institutional and network perspective on the content and consequences of TQM adoption. *Administrative Science Quarterly* 42 (2): 366–394.
- Westphal, J. D., M. D. L. Seidel, and K. J. Stewart. 2001. Second-order imitation: Uncovering latent effects of board network ties. *Administrative Science Quarterly* 46 (4): 717–747.
- Wilson, R. 2009. An examination of corporate tax shelter participants. *The Accounting Review* 84 (3): 969–999.

Zaheer, A., R. Gözübüyük, and H. Milanov. 2010. It's the connections: The network perspective in interorganizational research. *The Academy of Management Perspectives (formerly The Academy of Management Executive, AMP)* 24 (1): 62–77.

Zajac, E. 1988. Interlocking directorates as an interorganizational strategy: A test of critical assumptions. *Academy of Management Journal* 31 (2): 428–438.

APPENDIX A

Variable Definitions^a

Tax Avoidance and Control Variables (Compustat mnemonic in parentheses):

<i>CETR3</i>	three-year cash ETR, defined as the sum of cash taxes paid (TXPD) divided by the sum of pretax income (PI) excluding special items (SPI) in years <i>t</i> to <i>t</i> +2, following Dyreng et al. (2008);
<i>GETR3</i>	three-year GAAP ETR, defined as the sum of total tax expense (TXT) divided by the sum of pretax income (PI) in years <i>t</i> to <i>t</i> +2, following Dyreng et al. (2008);
<i>GP3</i>	three-year gross profit, defined as the sum of gross profit (GP) divided by the sum of sales (SALE) in years <i>t</i> to <i>t</i> +2;
<i>RET3</i>	three-year buy-and-hold returns for the years <i>t</i> to <i>t</i> +2;
<i>RCETR3_IND</i>	multinomial variable with values from 1 to 5 for firms ranked in the lowest to highest period-quintiles of industry-adjusted <i>CETR3</i> ;
<i>SIZE3</i>	three-year size measure, defined as the mean of the natural log of total assets (AT) in years <i>t</i> to <i>t</i> +2;
<i>ROA3</i>	three-year measure of return on assets equal to the sum of earnings before tax and special items (PI-SPI), divided by the sum of total assets (AT) in years <i>t</i> to <i>t</i> +2;
<i>LEV3</i>	three-year measure of leverage equal to the sum of long-term debt (DLTT), scaled by the sum of total assets (AT) in years <i>t</i> to <i>t</i> +2;
<i>PPE3</i>	three-year measure of property and equipment equal to the sum of gross property, plant, and equipment (PPEGT), scaled by the sum of total assets (AT) in years <i>t</i> to <i>t</i> +2;
<i>INTAN3</i>	three-year measure of intangible assets equal to the sum of intangible assets (INTAN), scaled by the sum of total assets (AT) in years <i>t</i> to <i>t</i> +2;
<i>RD3</i>	three-year measure of research and development equal to 1 if research and development expense (XRD) is non-zero and non-missing in any year during <i>t</i> to <i>t</i> +2, 0 otherwise;
<i>AD3</i>	three-year measure of advertising expense equal to the sum of advertising expense (XAD), scaled by the sum of sales (SALE) in years <i>t</i> to <i>t</i> +2;
<i>NOL3</i>	indicator variable equal to 1 if the firm reported a net operating loss (TLCF) in any year during <i>t</i> –1 through <i>t</i> +2, 0 otherwise;
<i>FOR3</i>	three-year measure of foreign income equal to the sum of foreign earnings (PIFO), scaled by the sum of total assets (AT) in year <i>t</i> to <i>t</i> +2; and
<i>SPI3</i>	three-year measure of special items equal to the sum of special items (SPI), scaled by the sum of sales (SALE) in year <i>t</i> to <i>t</i> +2.

Network Ties Variables

<i>TIES</i>	total number of firms that the focal firm is connected to via board interlocks;
<i>Low-tax ties (raw)</i>	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where <i>low-tax</i> firms are those in <i>RCETR3_IND</i> = 1;

(continued on next page)

APPENDIX A (continued)

<i>L-T TIES</i>	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks scaled by the firm's total board interlocks (<i>low-tax ties/TIES</i>). All variants of <i>L-T TIES</i> that follow are scaled by total <i>TIES</i> ;
<i>L-T TIES IND_{sim}</i> (<i>L-T TIES IND_{dissim}</i>)	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where the tied to firm is in the same (different) industry, where industry classifications follow those in Barth et al. (1998);
<i>L-T TIES IncomeMobile_{sim}</i> (<i>L-T TIES IncomeMobile_{dissim}</i>) (<i>L-T TIES IncomeMobile_{neutral}</i>)	number of similar <i>low-tax</i> firms the focal firm is connected to via board interlocks where a focal firm and its network tie are considered similar if they exhibit similar levels of <i>IncomeMobile</i> (high/high or low/low). High (low) <i>IncomeMobile</i> is defined as values in the top (bottom) 20 percent of the sample-period distribution of <i>IncomeMobile</i> , which equals the sum of the period ranks for R&D expense, advertising expense, percent foreign sales, percent gross profit plus 4 for SIC codes 283, 357, 367, 737, 738, following De Simone and Stomberg (2012). Network ties are considered dissimilar if they exhibit dissimilar levels of <i>IncomeMobile</i> (high/low or low/high). Remaining interlocks are classified as neutral ties;
<i>L-T TIES Growth_{sim}</i> (<i>L-T TIES Growth_{dissim}</i>) (<i>L-T TIES Growth_{neutral}</i>)	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where a focal firm and its network tie are considered similar if they exhibit similar growth opportunities (high/high or low/low). High (low) growth is defined as having a three-year average book-to-market ratio ($CEQ/PRCC\_F * CSHO$) in the top (bottom) 20 percent of the sample period distribution. Network ties are considered dissimilar if they exhibit dissimilar levels of growth (high/low or low/high). Remaining interlocks are classified as neutral ties;
<i>L-T TIES Risk_{sim}</i> (<i>L-T TIES Risk_{dissim}</i>) (<i>L-T TIES Risk_{neutral}</i>)	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where a focal firm and its network tie are considered similar if they exhibit similar risk (high/high or low/low). A firm is identified as high (low) risk if during the period it is above (below) the median for three or more of the following metrics: three-year standard deviation of returns; three-year standard deviation of cash flows; three-year standard deviation of ROA; and three-year average beta, Z-score, standard deviation of EBIT, and leverage. Network ties are considered dissimilar if they exhibit dissimilar levels of risk (high/low or low/high). Remaining interlocks are classified as neutral ties;
<i>L-T TIES_{EXEC}</i> (<i>L-T TIES_{NONEXEC}</i>)	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where the shared director is (is not) the CEO or CFO of one of the interlocked firms;
<i>L-T TIES local AUDITOR_{same}</i> (<i>L-T TIES local AUDITOR_{diff}</i>)	number of <i>low-tax</i> firms the focal firm is connected to via board interlocks where the focal firm and its network partner engage the same (different) audit firm in the same metropolitan statistical area (MSA);
<i>L-T10 TIES</i>	number of <i>long-term low-tax</i> firms the focal firm is connected to via board interlocks scaled by the total number of firms the focal firm is connected to, scaled by the total number of firms that the focal firm is connected to via board interlocks where <i>long-term low-tax</i> firms are those in lowest quintile based on ranks of ten-year industry-adjusted cash effective tax rate as measured in Dyreng et al. (2008);
<i>High-ROA TIES</i>	number of <i>high-ROA</i> firms the focal firm is connected to via board interlocks scaled by the firm's total board interlocks, where <i>high-ROA</i> firms are those in top quintile of <i>ROA3</i> by industry and period;

(continued on next page)

APPENDIX A (continued)

<i>High-GP TIES</i>	number of <i>high-GP</i> firms the focal firm is connected to via board interlocks scaled by the firm's total board interlocks, where <i>high-GP</i> firms are those in top quintile of three-year gross profit by industry and period;
<i>High-Return TIES</i>	number of <i>high-Return</i> firms the focal firm is connected to via board interlocks scaled by the firm's total board interlocks, where <i>high-Return</i> firms are those in top quintile of three-year buy-and-hold returns by industry and period; and
<i>High-Tax TIES</i>	number of <i>high-tax</i> firms the focal firm is connected to via board interlocks scaled by the firm's total board interlocks (<i>high-tax ties/TIES</i>), where <i>high-tax</i> firms are those in <i>RCETR3_IND</i> = 5.

^a Each of the variables is constructed as an average over a three-year period. There are 12 rolling three-year periods in our sample timeframe (1996–2009).

Mispricing of Book-Tax Differences and the Trading Behavior of Short Sellers and Insiders

Sabrina S. Chi

University of Arkansas

Morton Pincus

Siew Hong Teoh

University of California, Irvine

ABSTRACT: We find evidence that investors misprice information contained in book-tax differences (BTDs), measured as the ratio of taxable income to book income, TI/BI . Low TI/BI predicts worse earnings growth and abnormal stock returns than high TI/BI . We find that short sellers and insiders arbitrage BTD mispricing, but the arbitrage is imperfect because of constraints on short selling and insider trading. Under SFAS No. 109 the predictability is stronger for $TEMP/BI$, the temporary component of TI/BI , which reflects greater managerial discretion. The results are incremental to a large set of known accruals-based anomaly predictors. We suggest that a sunshine policy of disclosing a reconciliation of book and taxable incomes can reduce mispricing of BTDs and improve capital market resource allocation.

Keywords: *book-tax differences; arbitrage; market efficiency; anomalies; mispricing; short selling; insider trading.*

Data Availability: *Data are obtained from the public sources as indicated in the text.*

We appreciate helpful comments from John Harry Evans III (senior editor) and two anonymous referees, Michael Donohoe (AAA Annual Meeting discussant), Arthur Francia, Michelle Hanlon, Bob Lipe, Richard Sloan, Bill Schwartz, Jake Thornock, Sam Tiras (FARS Conference discussant), Ashley Wang, Frank Zhang, Oklahoma State University Research Conference participants, UCI Accounting Doctoral Seminar participants, and workshop participants at California Polytechnic University, San Luis Obispo, University of Houston, Lehigh University, Nanyang Technological University, National University of Singapore, and Santa Clara University on this version and an earlier version entitled “Do Short Sellers and Insiders Profit from the Mispricing of Book-Tax Differences?” We also thank Xuan Huang and Peng-Chia Chiu for expert research assistance.

Editor’s note: Accepted by John Harry Evans III.

Submitted: January 2012

Accepted: September 2013

Published Online: October 2013

I. INTRODUCTION

Book income (*BI*) as reported in a firm's financial statements generally differs from taxable income (*TI*) as reported to the tax authorities. This difference is commonly referred to as the book-tax difference (BTD). In this study, we provide a comprehensive analysis of the information contained in BTDs for investors. Specifically, we investigate whether *TI/BI* and particularly its temporary component, denoted *TEMP/BI*, which reflects greater managerial discretion than *TI/BI*, contain useful information about future earnings growth that is mispriced by investors. We find evidence that *TI/BI* and *TEMP/BI* predict future earnings growth and future abnormal returns. We then examine whether short arbitrageurs and firm insiders trade on personal account in a manner consistent with arbitraging the mispricing of *TI/BI* and *TEMP/BI*. We find evidence consistent with short sellers and insiders profiting from unsophisticated investors' mispricing of *TI/BI* and especially *TEMP/BI*.¹ Finally, we discuss how accounting disclosure for income taxes akin to a "sunshine policy" may reduce mispricing of BTDs by less sophisticated investors and improve capital market efficiency.

Our topic is relevant to researchers in three popular areas, namely accounting for income taxes, earnings management, and capital market anomalies. Graham, Raedy, and Shackelford (2012, 412) note that research on the "accounting for income taxes has become the most active area of accounting research in taxation." Earnings management and accounting anomalies continue to be important research areas because of their direct implications for capital market participants, firm decision makers, and accounting rule-setters.

BTD variables may contain information about future firm fundamentals. Dechow, Ge, and Schrand (2010) argue that generally accepted accounting principles (GAAP) restrictions on the choice of accounting standards often result in book income being an imperfect measure of firm performance. Dhaliwal, Lee, Pincus, and Steele (2012) take a similar perspective and suggest that book income and taxable income, as alternative performance measures, supplement each other so that together they provide more information about future earnings and firm valuation than each does by itself. This is analogous to how accruals contain information that is incremental to future fundamentals when compared with either book income or cash flows from operations individually.² In addition, all else equal, we expect managers to minimize *TI* and maximize *BI* (Hanlon, Laplante, and Shevlin 2005; Badertscher, Phillips, Pincus, and Rego 2009).³ Thus, high *TI/BI* values in the cross-section likely indicate higher future earnings growth, whereas relatively low *TI/BI* values indicate lower earnings growth.

Limited attention, arising from information-processing costs or limited cognitive power, can cause BTD variables to be mispriced. Investors with limited attention likely use simple heuristics such as a price/earnings ratio to value the firm, and ignore relevant hard-to-estimate or hard-to-process items (Hirshleifer and Teoh 2003). BTDs, in addition to reflecting firm future fundamentals, may also capture earnings management and/or tax management. Valuing BTDs

¹ For practical reasons, we study only short sellers and insiders as representatives of sophisticated investors, and generally ignore trading behavior of more sophisticated institutional investors such as some hedge funds and the firm itself (via repurchases). Throughout we also refer to unsophisticated investors as general, average, or inattentive investors.

² Taxable income uses a hybrid of cash and accrual bases, and is not simply a proxy for cash flows from operations (Lev and Nissim 2004, 1045, 1070–1072; Hanlon et al. 2005, footnote 14, p. 414). Thus, we expect that BTD variables are not simply proxies for accruals, but because BTD variables are related to accruals, we control for all known accrual-related predictors in our tests.

³ We expect *TI/BI* to average less than 1.0. Weber (2009) reports mean *TI/BI* (denoted *TAX*) of 0.893 for 1984–2004. From Lev and Nissim (2004) we estimate a mean *TI/BI* of 0.791 from their Table 1, Panel B for 1993–2000. We report a mean (median) *TI/BI* of 0.659 (0.724) and 0.567 (0.813) for our short interest and insider trading samples, respectively (Table 2, Panel A).

demands considerable cognitive and processing resources to estimate taxable income and isolate unbiased implications of BTDs for future cash flows. Thus, investors with limited attention and processing resources may misestimate future cash flows and discount inappropriately for the agency incentives of managers to behave opportunistically, and so misprice the firm.

Researchers suggest that the temporary component of BTDs reflects greater managerial discretion than the permanent component (Phillips, Pincus, and Rego 2003; Hanlon 2005; Blaylock, Shevlin, and Wilson 2012).⁴ Evidence in Phillips et al. (2003) suggests temporary BTDs are informative about pre-tax earnings management, and Hanlon (2005) finds that earnings, and specifically accruals, are less persistent when firms have large temporary BTDs. Hanlon (2005) also reports some evidence of anomalous pricing for large temporary BTDs. Thus, analogous to the greater mispricing of discretionary accruals than normal accruals (Teoh, Welch, and Wong 1998a; Xie 2001), we hypothesize that *TEMP/BI* rather than *PERM/BI*, the permanent component of *TI/BI*, is more likely to be mispriced.

Our tests of the predictability of earnings growth and of abnormal stock returns improve on past studies in several ways. Lev and Nissim (2004) are the first to report that *TI/BI* predicts future earnings growth and abnormal stock returns in the pre-SFAS No. 109 (FASB 1992) period of 1973–1992. However, the predictability for stock returns is statistically significant in the post-SFAS No. 109 period, which spanned 1993–2000 in their sample, only after observations for 1998 are removed. Lev and Nissim's (2004, Table 5) results indicate that temporary BTDs in their sample do not incrementally predict earnings growth or abnormal stock returns, before or after SFAS No. 109. Weber (2009) reports stock return predictability for *TI/BI* only in the low-analyst-following subsample, and weaker results under SFAS No. 109, which ends in 2004 in his study; he does not consider *TEMP/BI*.

We systematically examine predictability of future earnings growth and future abnormal returns using *TI/BI*, *TEMP/BI*, and *PERM/BI*. We estimate *TI* as income tax expense grossed up by the corporate income tax rate minus the change in net operating loss carryforwards and we estimate *TEMP* as the deferred tax expense grossed up by the corporate tax rate. We multiply *TI* and *TEMP* by 1 minus the corporate tax rate. *PERM* is the residual after subtracting the temporary component from estimated *TI* (see Section III).

Lev and Nissim (2004) argue that SFAS No. 109, instituted in December 1992, affords managers greater discretion in accounting for deferred taxes and permits greater earnings management.⁵ Their sample period ends in 2000 when the dot-com bubble burst. Our sample period (1988–2009) has more post-2000 observations, which reduces the impact of the dot-com bubble period on the analysis. Unlike Lev and Nissim (2004), we also include non-December fiscal year-end firms, which comprise 37 percent of our sample.

Our test design improves on past studies by including size, book-to-market, and momentum as risk controls, and by including abnormal accruals, percent operating accruals, total accruals, tax momentum, and Piotroski's (2000) fundamental *F-Score* as controls for other known accounting predictors of future earnings and returns. It is important to investigate incremental earnings and stock return predictability of the BTD variables beyond accrual-related predictors because BTDs are

⁴ Hanlon and Heitzman (2010, 131) state that since “accounting accruals reflect more discretion than the tax laws allow, temporary differences between book and tax income reveal something about discretion in non-tax accounting accruals (bad debt accrual, warranty expense, deferred revenue, etc.),” but this generally does not extend to permanent differences, since they are not driven by the accounting accruals process.

⁵ Deferred income taxes under SFAS No. 109 require estimates of future tax rates and adjustments for deferred tax assets that are unlikely to yield future tax benefits. The greater discretion for estimates allowed by SFAS No. 109 likely imposes greater attention demands on investors when evaluating reported earnings, and so we expect a greater possibility of mispricing of BTD information after 1993 under SFAS No. 109.

related to accruals, and accrual-related variables are already known predictors of earnings and returns in the literature.

We examine robustness of results for stock return predictability using both the rolling monthly cross-sectional Fama-MacBeth panel regression method and the characteristics-adjusted portfolio hedge return method. In the latter method, we double sort *TI/BI* or *TEMP/BI* with prior known accounting predictors of returns to estimate incremental hedge profits for *TI/BI* and *TEMP/BI*. These extensive controls and robustness test methods provide more conclusive evidence that our BTD measures have incremental predictability. Finally, we also examine return predictability of BTDs in subsamples grouped by the intensity of analyst following.

We find robust evidence that *TI/BI* incrementally predicts future earnings growth and future abnormal stock returns in both the pre- and post-SFAS No. 109 periods (hereafter, pre-109 and post-109, respectively). A *TI/BI* trading strategy earns 72 basis points per month, which is approximately 9 percent annualized, adjusted for standard risk characteristics over our sample period. Further, the magnitude of the *TI/BI* hedge profits is substantial, representing approximately two-thirds that of the abnormal accruals hedge profit, and it contributes an incremental 0.5 percent per month or more than 6 percent annualized hedge profit over abnormal accruals so that a joint abnormal accruals-*TI/BI* trading strategy earns 1.59 percent per month, which exceeds 20 percent annually. The relatively small overlap in the predictability of *TI/BI* with abnormal accruals is consistent with abnormal accruals alone being an insufficient proxy for all earnings management. Most important, the predictability of both future earnings and future stock returns comes from *TEMP/BI*, the higher discretionary component. Unlike prior weak or ambiguous evidence on *TEMP/BI*, we find strong evidence of earnings and returns predictability for *TEMP/BI* post-109. In particular, the magnitude of *TEMP/BI* hedge profits is almost three times that of *PERM/BI*.

The evidence that *TI/BI* (and *TEMP/BI*) is mispriced motivates the next set of tests on the trading behavior of sophisticated investors and insiders. Sophisticated investors are more likely to attend to BTD information contained in financial statements that is ignored by the average less-attentive investor. Therefore, sophisticated investors can profit by short selling overpriced stocks, which are firms with low *TI/BI*, and by construction high *TEMP/BI*, to exploit the overpricing. Thus, we test whether short selling is related to BTD variables.

Short selling is the sale of securities that one does not own but borrows from institutional investors, brokerages, or broker-dealers with the intention of buying back at a later date at a lower price to return the shares to the lenders. Critics argue that short selling encourages traders to manipulate the market, leading to price distortions, increased volatility, and a loss of investor confidence.⁶ Proponents, on the other hand, argue that short selling facilitates market efficiency when short arbitrageurs uncover overpriced stocks and help to correct the mispricing through shorting the firms' stocks. For example, James S. Chanos, a known short seller, was among the first to unearth problems in Enron's financial reports.⁷

⁶ In 2005 Amr I. Elgindy was convicted of bribing Federal Bureau of Investigation agents to obtain confidential information and then establishing short positions in companies under investigation by the FBI, and was convicted of leaking the information via the Internet to help ensure that prices of those companies' shares fell after he took short positions (U.S. Attorney's Office, Eastern District of New York [USAO EDNY] 2006). Some critics blame short selling for driving some of the leading financial institutions to the edge of collapse in the financial crisis of 2008. See Lauricella (2009), Fidler (2010), Securities and Exchange Commission (SEC 2010), and Thomas (2010) for a description of new short sale regulations.

⁷ See Chanos (2008), who found the problematic "gain on sale" accounting method for long-term energy trades, in which firms recognize gains up-front, estimated as the present value of the future profits from the energy trades made today, and various related-party transaction disclosures in Enron's 1999 Form 10-K and 2000 Form 10-Qs, and profited from the information.

Our study of the relation between short selling and BTDs contributes to the literature first by providing indirect confirmation of the primary result that BTDs capture predictive information. Second, we add BTD measures to the list of accounting items that academic studies report short arbitrageurs use to detect likely upward earnings management and overvaluation.⁸ Evidence of mispricing of an accounting item can encourage accounting rule-setters to evaluate and improve the effectiveness of current reporting rules, managers to evaluate more effective communication methods to investors, analysts to improve investor understanding of the accounting item, and investors to pay greater attention to the accounting item when valuing the firm. Third, our study also contributes to the asset-pricing literature debate concerning the appropriate model for expected returns estimation. Market inefficiency skeptics often challenge tests that demonstrate predictability of abnormal returns on the premise that risk premia have been mismeasured. Evidence that short selling is related to *TI/BI* and *TEMP/BI* helps to corroborate the claim that *TI/BI* and *TEMP/BI* are mispriced, not that risk is mismeasured.⁹

We adopt the statistical method in Hirshleifer, Teoh, and Yu (2011) who examine short arbitrage of the accrual anomaly using an extensive set of controls for risk and arbitrage costs. We find strong evidence of short arbitrage of *TI/BI* and *TEMP/BI*, consistent with the overpricing of these ratios. Short interest, which measures open short positions of stocks, decreases by an economically large 33 percent moving from the lowest to the highest quintile of *TI/BI*.

A natural question that arises when there is evidence of short arbitrage is how effectively short arbitrage corrects the mispricing of the predictor variables. Hirshleifer et al. (2011) and Stambaugh, Yu, and Yuan (2012) view the return difference between the long side and short side of trading strategies as indicative of the severity of short sale constraints. It is harder or more costly to sell a stock short than to go long, so arbitrageurs can more easily exploit underpricing than overpricing. The asymmetry in returns between the long and short sides of an anomaly is therefore a measure of the effectiveness of arbitrage. Following Hirshleifer et al. (2011), we measure the return asymmetry as the negative of the sum of the abnormal returns between the long and short sides of the *TI/BI* and *TEMP/BI* anomalies. We find significantly large return asymmetry for *TI/BI* and *TEMP/BI*, which indicates that mispricing of *TI/BI* and *TEMP/BI* is not fully eliminated because short arbitrage is costly.

Our final set of tests considers insider trading, which has long been a concern to U.S. securities regulators (Pulliam, Rothfeld, Strasbur, and Zuckerman 2010). Insiders are firm employees, such as company officers and directors, and those with privileged access to private information about the firm, including large blockholders, consultants, auditors, and lawyers. Insiders can buy or sell but not short their own firm's stocks on personal account, with some restrictions under federal law to safeguard outside shareholders from unfair advantage because insiders may possess private information about the firm. The prohibitions against insider trading therefore relate to whether insiders trade on information as yet unknown to outsiders, and past academic studies find that insider trades reflect superior private knowledge.¹⁰

⁸ Evidence of short arbitrage of accounting-related information includes Dechow, Hutton, Meulbroek, and Sloan (2001) for stocks with low fundamental-to-price ratios; Desai, Krishnamurthy, and Venkataraman (2006) for restatement announcements; Karpoff and Lou (2010) for SEC enforcement actions; Hirshleifer et al. (2011) for accruals; and Drake, Rees, and Swanson (2011) for analyst recommendations.

⁹ We include all standard asset-pricing risk controls in our Fama-MacBeth panel regressions of future returns on the predictor variables, and use alternative test designs, specifically, characteristics-adjusted portfolio hedge profits. We use the standard inference in the large asset-pricing literature that the predictor variables are mispriced when we find evidence that the predictor variables incrementally predict future returns with statistical significance. Also see Weber (2009).

¹⁰ Research has found persistent evidence of profitable insider trades. Insider trades appear to reflect superior knowledge of future earnings and returns (Piotroski and Roulstone 2005), occur in strategic settings such as following the analyst walk-down of forecasts to beatable levels (Richardson, Teoh, and Wysocki 2004), and occur as early as three to nine quarters in advance of breaks in a string of consecutive earnings increases (Ke, Huddart, and Petroni 2003).

However, our tests relating BTDs to insider trading are designed to determine whether insider trades take advantage of investors' inability to process the information contained in accounting items related to BTDs efficiently. Since this information is already released to outsiders in the financial reports, we do not test whether insiders trade illegally. Because insiders have private information that substantially resolves uncertainty about future earnings, if the market inefficiently responds to the release of financial statement information, then insiders can observe that and profit by trading against the mispricing. We test whether insiders exploit *TI/BI* (*TEMP/BI*) mispricing by net selling firms with low *TI/BI* and high *TEMP/BI* on personal account after financial statements are released. The results are statistically and economically significant and indicate that officers and directors are net sellers of their own firm's equity in firms with low *TI/BI* (high *TEMP/BI*), consistent with managers' trading as if to profit from investor mispricing of BTDs. To the best of our knowledge, this is the first study to relate BTD measures to short selling and insider trading.

In sum, our research contributes to research on capital market anomalies, book-tax differences, short selling, and insider trading. Collectively, our evidence suggests that *TI/BI* estimated from reported accounting numbers contains information about future firm performance that general investors do not appear to understand and that short arbitrageurs and managers appear to exploit. A sunshine policy that increases transparency by requiring firms to disclose a reconciliation of book and taxable incomes may, subject to costs and other considerations, benefit the average investor by reducing mispricing of BTD information and so improve resource allocation in the capital market. Furthermore, past studies argue that temporary BTDs reflect greater managerial discretion and opportunistic behavior. Showing that predictability of future fundamentals and stock returns are greatest and short arbitrage and insider net sales are highest for *TEMP/BI* is thus consistent with the presence of greater managerial discretion. Further, this raises the possibility that managers may choose financial and tax reporting opportunistically and trade on personal account to profit from the mispricing of BTD information. A sunshine policy as we described would also help constrain such an agency problem.

Section II discusses institutional features of BTDs, short interest, and insider trades. Section III describes data and variable measurement and Section IV reports the research design and results on earnings growth and stock return predictability. Section V presents our short interest analyses, and Section VI reports our insider trading analyses. We discuss additional sensitivity checks on the results in Section VII, and Section VIII concludes.

II. INSTITUTIONAL FEATURES OF BOOK-TAX DIFFERENCES, SHORT INTEREST, AND INSIDER TRADING

Book-Tax Differences (BTDs)

Book income is determined by GAAP, whereas taxable income is governed by the Internal Revenue Code, and Section 446 (a) states, "taxable income shall be computed under the method of accounting on the basis of which the taxpayer regularly computes his income in keeping his books." While both book and taxable incomes typically reflect accrual accounting, they are computed for different purposes and therefore there are differences in their respective accounting standards and rules. Financial accounting seeks to provide investors and other stakeholders with information useful for assessing firm value and managerial stewardship whereas taxable income is computed primarily to determine firms' tax liabilities, with some tax rules serving as incentives for certain types of investments and activities.

Differences between book and tax accounting for various transactions give rise to either permanent or temporary differences. Permanent differences occur when an item affects taxable income but never affects book income, or *vice versa*, such as interest revenue on municipal bonds,

which increases book income but never affects taxable income. Temporary differences arise when the book and tax treatments for a transaction differ in a given year, but have the same cumulative effect over the life of the firm (ignoring the time value of money). For example, book depreciation is calculated based on the estimated service life of a fixed asset, whereas tax depreciation follows specified depreciation rates.

Temporary BTDs reflect future taxable and deductible amounts. Future taxable amounts create (or increase) deferred tax liabilities and require recognition of deferred tax expense. In contrast, future deductible amounts create (or increase) deferred tax assets and thereby recognize deferred tax benefits; they also reduce deferred tax expense. All else equal, an increase in net deferred tax liabilities is consistent with a firm currently recognizing revenue and/or deferring expense for book purposes relative to its tax reporting such that $BI > TI$. Similarly, an increase in net deferred tax assets, or equivalently, a decrease in net deferred tax liabilities, is consistent with a firm currently recognizing expense and/or deferring revenue for book relative to taxable income so that $BI < TI$. Because temporary BTDs generally reflect greater discretion to manage BI relative to TI , temporary BTDs are an indicator of earnings management (Phillips et al. 2003). Permanent BTDs are generally less common and less subject to discretion. Because they are unlikely to be driven by the accrual process, they are less likely to reflect significant earnings management (Hanlon and Heitzman 2010). This reasoning leads us to predict that a TI/BI pricing anomaly is more likely a result of mispricing of $TEMP/BI$.

Short Selling

Rule 3b-3 of the Securities Exchange Act of 1934 defines a short sale as “any sale of a security which the seller does not own or any sale, which is consummated by the delivery of a security borrowed by, or for the account of, the seller.” Thus, to sell short, an investor must borrow shares from another investor who owns them and is willing to lend. Short arbitrage is risky and costly. The short seller typically leaves cash collateral, equal to 102 percent of the market value of the borrowed shares, with the lender. The lender pays the short seller interest, referred to as the rebate rate, on the collateral. The spread between the rebate rate and the market interest rate on cash funds, referred to as the loan fee, is a direct cost to the short seller. Current regulations allow the lender to recall a loan. If recalled, the borrower covers the short by buying back the shares and returning them to the lender, or reestablishes the short at a higher loan fee. An additional source of risk is a rapid increase in stock price, a “short squeeze,” that can occur when short sellers cover their positions. As this involves buying shares, a short squeeze can cause a further rise in the stock’s price and trigger additional margin calls and short covering.¹¹

While controversial, short selling is in principle a legitimate trading strategy. It is a way to trade on bad news about a firm, which should help stock prices more fully reflect value-relevant information. As such, it can serve to make share prices more efficient. Short sellers assume the risk that they will be able to buy the stock at a more favorable price than the price at which they sold short, just as investors on the long side risk being able to sell their shares later at a more favorable price than what they paid to purchase them. However, while the maximum loss from going long is the amount invested, the loss is unbounded when shorting a stock.

To test if there is short arbitrage of TI/BI , we examine whether short interest is high for firms with low TI/BI . Since arbitrage is easier when the supply of loanable shares proxied by institutional ownership is greater, we also test whether the expected negative relation between short interest and

¹¹ NASDAQ’s short sale rule prohibits members from selling a NASDAQ National Market stock at or below the inside best bid when that price is lower than the previous inside best bid in that stock. The inside best bid is the highest bid price among all competing market makers in a NASDAQ security.

TI/BI is stronger in high institutional ownership firms. Finally, since temporary BTDs typically reflect more discretion and reverse over time, in contrast to permanent BTDs, we expect greater short arbitrage when *TEMP/BI* is large than when *PERM/BI* is large.

Insider Trading

The U.S. Supreme Court ruled in 1909 that a director who buys his firm's stock when he knows the price is about to jump and does not disclose his private information has committed fraud by buying. In 1912–1913, the Pujo Committee failed to regulate insider trading but noted, “the scandalous practices of officers and directors in speculating upon inside and advance information as to the action of their corporations may be curtailed if not stopped” (Dolgoplov 2008). The 1933 and 1934 Securities Acts prohibit short-swing profits by insiders and SEC Rule 10b-5 prohibits fraud related to securities trading. The Insider Trading Sanctions Act of 1984 and the Insider Trading and Securities Fraud Enforcement Act of 1988 provide for penalties for illegal insider trading as high as three times the profit gained/loss avoided from such trading.¹²

In response to the insider trading laws, corporations have voluntarily established guidelines of behavior to safeguard the manager when he trades his firm's stock. These voluntary restrictions often take the form of explicit blackout periods, such as periods prior to earnings announcement dates when insiders are not permitted to trade (Richardson et al. 2004). As a result, insider trades are often concentrated in the several weeks immediately after an earnings announcement when there is no blackout period and information asymmetry between insiders and outsiders is minimized. We thus examine insider trading in the 20-trading day window immediately following earnings releases.

III. DATA AND VARIABLE MEASUREMENT

Measurement of BTD Variables

Following Lev and Nissim (2004), we estimate *TI/BI*, the tax fundamental variable that captures total BTDs, as follows:

$$\frac{TI}{BI} = \frac{\text{After tax Taxable Income}}{\text{After tax Book Income}},$$

where Table 2 summarizes all variable definitions and data sources. After tax book income is measured as net income before extraordinary items. We restrict the sample to include only positive *BI* observations. We estimate after tax taxable income by first grossing up the current portion of the reported income tax expense using *tx*, the top statutory corporate tax rate (i.e., current tax expense/*tx*), subtracting the change in net operating loss carryforwards (Hanlon, Krishnan, and Mills 2012), and then multiplying by $(1 - tx)$:

$$TI = \left[\frac{\text{Current Income Tax Expense}}{tx} - \Delta NOL \right] * (1 - tx).$$

As in previous research (Gleason and Mills 2002), the current portion of income tax expense is the sum of current federal and foreign income tax expenses, but when either of these amounts is missing, it is the difference between total income tax expense and the deferred portion of the

¹² Recent high-profile criminal convictions of insider trading cases include Martha Stewart in 2003 and Raj Rajaratnam in 2011, who was sentenced to 11 years in jail, the longest term in the history of insider trading convictions (Pulliam and Bray 2011).

income tax expense. From 1993 onward tx equals 35 percent (34 percent for 1988–1992). TI/BI ratio values less than one imply that current TI is less than current BI , and relatively low TI/BI values suggest that current BI is unlikely to be sustainable.

BTD, the difference between BI and TI , has a permanent and temporary component:

$$BTD = [BI - TI] = TEMP + PERM.$$

Following Hanlon (2005), we estimate $TEMP$ as:

$$TEMP = \left[\frac{\text{Deferred Tax Expense}}{tx} \right] * (1 - tx).$$

Deferred tax expense (DTE) is the sum of deferred federal and foreign tax expenses, but when either of these amounts is missing, DTE is the deferred portion of total income tax expense. Under SFAS No. 109, DTE is the change of firms' deferred tax assets and liabilities during the current year. Consistent with the calculation of TI/BI on an after tax basis, we multiply by $(1 - tx)$ to express $TEMP$ on an after-tax basis. We calculate the permanent component as the difference between total BTDs and temporary differences, $PERM = [BI - TI] - TEMP$. By construction, decreases in TI/BI reflect increases in $TEMP/BI$ and/or $PERM/BI$.¹³

Measurement of Previously Known Accounting Predictors and Other Key Variables

We control for these previously known accounting predictors: abnormal accruals (Teoh et al. 1998a, 1998b), percent operating accruals (Hatzalla, Lundholm, and Van Winkle 2011), and tax momentum (Thomas and Zhang 2011), in addition to fundamentals-related variables reflected in Piotroski's F -Score (Piotroski 2000), and earnings persistence-related variables linked to BTDs (Guenther 2011). First, we compute abnormal accruals using the cross-sectional modified Jones model (Jones 1991; Dechow, Sloan, and Sweeney 1995). Using cash flow statement data we estimate accruals as the difference between income from continuing operations and cash flows from operations (Hribar and Collins 2002), $TAcc_{jt} = EBEI_{jt} - (CFO_{jt} - EIDO_{jt})$, where $TAcc$ is total accruals, $EBEI$ is income before extraordinary items, CFO is cash flows from operations, and $EIDO$ is extraordinary items and discontinued operations included in CFO . We then estimate the following regression for each two-digit SIC-year combination having at least 15 observations:

$$TAcc_{jt} = \beta_0(1/Assets_{j,t-1}) + \beta_1(\Delta Sales_{jt} - \Delta AR_{jt}) + \beta_2PPE_{jt} + v_{jt}, \quad (1)$$

where $Assets$ is total assets, $\Delta Sales$ is the change in sales, ΔAR is the change in accounts receivable from operating activities, and PPE is gross property, plant, and equipment. All variables are scaled by beginning-of-year total assets. Normal accruals and abnormal accruals ($AbnAcc$) are respectively the fitted values and residuals from regression (1).

Second, following Hatzalla et al. (2011), we compute percent operating accruals as the difference between net income and cash from operations, scaled by the absolute value of net income. Third, Thomas and Zhang (2011) find that quarterly tax momentum is associated with

¹³ Lev and Nissim (2004, 1042) state that "while most previous studies focus on a single tax-related component—temporary differences, permanent differences, or tax accruals ... our tax fundamental captures ... all three tax components, creating a potentially powerful earnings quality indicator." Lev and Nissim (2004) also note that Compustat does not provide sufficient information to allow for the estimation of tax accruals. Tax accruals include changes in the deferred tax asset valuation allowance account, tax cushion reserves, and foreign income permanently reinvested. Hence, by construction tax accruals are included in $PERM/BI$. If tax accruals are non-trivial in amount and reflect strategic managerial discretion that investors are unable to discount appropriately, then our $PERM/BI$ variable would also predict stock returns from the presence of tax accruals.

anomalous pricing. We modify their *TaxMom* variable to annual data as the difference between tax expense per share in year t and year $t-1$ scaled by assets per share in year $t-1$.

Fourth, Piotroski’s (2000) annual *F-Score* measures a firm’s financial condition from a combination of firm fundamental variables, and is estimated as the sum of nine binary indicators:

$$F\text{-Score} = \sum \left(\begin{array}{l} \text{positive return of assets (ROA), positive CFO, positive change in ROA,} \\ \text{negative accruals, negative change in leverage (or no long term debt),} \\ \text{positive change in current ratio, no issuance of common equity,} \\ \text{positive change in gross margin, positive change in asset turnover} \end{array} \right).$$

F-Score ranges from 0 to 9 and high values indicate superior financial condition. Finally, following Guenther (2011), we control for age, special items, non-operating income, gain or loss reported on the income statement and on the statement of cash flows for the most recent fiscal year since these variables help explain links between large BTDs and earnings persistence.

Measurement of Short Interest

Short interest reflects the open short positions of stocks with settlements on the last business day on or before the 15th of each month for both NYSE- and NASDAQ-listed companies. It normally takes several days to settle a short sale, and the last date when a short sale trade occurs for a monthly record is called the trade date. Prior to June 1995, it was five days before the settlement date; it then became three days. To calculate short interest, for example, for NASDAQ stocks, member firms report monthly to the National Association of Securities Dealers Regulation, Inc.’s (NASDR) Customer Advocacy and Quality Management Department their short positions for all accounts, in shares, warrants, units, ADRs, and convertible preferred stocks. NASDR compiles short interest data for each security and publishes the data on the eighth business day after the reporting settlement date.

Monthly short interest data from NYSE and NASDAQ are available for the period 1988 through 2009. We calculate short interest, *SI%*, in a given firm-year as the short position reported by the NYSE or NASDAQ in the fifth month after the fiscal year-end divided by the number of shares the firm has outstanding as reported on CRSP for the same month, and then multiplied by 100 (Asquith, Pathak, and Ritter 2005; Hirshleifer et al. 2011). The four-month gap between the fiscal year-end and the short position date ensures that short sellers have the financial report information available to them prior to taking short positions.¹⁴

Estimation of the Effectiveness of Short Arbitrage Using Return Asymmetry

Short arbitrage is risky, and the cost of a short position is higher than the cost of taking a long position in a stock. The higher constraints to short arbitrage reduce an arbitrageur’s ability to correct overpricing relative to underpricing, and therefore create an asymmetry in returns between the long and short sides of an anomaly-related trading position. Following Hirshleifer et al. (2011), we estimate the effectiveness of short arbitrage related to an anomaly using the abnormal return

¹⁴ Following Hirshleifer et al. (2011), we select the short interest observation in the fifth month only to relate to each firm-year predictor observation (e.g., *TI/BI* in our study). Including all 12 monthly short interest observations induces serial correlation, and averaging across the future 12-month short interest observations imposes an unnecessary assumption that short arbitrageurs maintain their positions for 12 months. When we select the short interest observation in the fourth (sixth) month after the fiscal year-end as an alternative time window, the results remain robust in all of the tests.

asymmetry, calculated as $-(R_H + R_L)$, which is the negative of the sum of the returns of the top and bottom anomaly quintiles, R_H and R_L , respectively. The return $-(R_H + R_L)$ is interpreted as the mean return on a portfolio that is short on the highest and lowest anomaly quintiles. In the case of *TI/BI*, a larger absolute value of the abnormal returns of the low *TI/BI* portfolio (*L*) compared with those of the high *TI/BI* portfolio (*H*) increases $-(R_H + R_L)$. A larger asymmetry is therefore consistent with greater constraints on short selling and therefore less effective short arbitrage. We also control for other costs of arbitrage, as described in Section V.

Measurement of Insider Trading

The insider trading data are from Thompson Financial and contain all insiders’ stock purchases, sales, and option exercises reported in Forms 3, 4, and 5 as required by the SEC. We only include officers’ and directors’ stock sales and purchases transaction data.¹⁵ Following Richardson et al. (2004), we measure insider trading activities using *IT%*, a continuous variable computed as the number of shares sold net of purchases by insiders in the 20 trading days after earnings announcements scaled by the number of shares outstanding at fiscal year-end and then multiplied by 100 to express as a percentage.

Sample Selection

We select the sample by first merging the monthly CRSP stock returns file with the monthly short interest file from the NYSE or NASDAQ according to stock ticker and calendar month. We exclude: (1) foreign firms since they likely follow different accounting standards and tax laws; (2) financial services and utility industry firms, which have different reporting requirements; (3) mutual funds, trusts, real estate investment trusts, limited partnerships, and other flow-through entities, since these enterprises do not report income taxes; and (4) loss firms, because computing and interpreting *TI/BI* for loss firms is problematic.¹⁶ The sample is then matched with the annual Compustat file, where firms must have sufficient Compustat data to compute regression variables, as explained below, and must also have necessary stock returns and volume data from CRSP. As noted above, Thompson Financial is the source for the insider trading data and we obtain data for the same period, 1988–2009, as for our short sales sample.

Descriptive Statistics

The sample selection process is summarized in Table 1, Panel A for the short interest sample, and in Panel B for the insider trading sample. There are 78,320 firm-year observations for the analysis on short interest; a firm-year observation is drawn from the fifth month in the fiscal year. The insider trading sample is comprised of 47,291 observations.

Table 2, Panel A presents descriptive statistics for the main variables in each sample. The mean (median) *TI/BI* is 0.659 (0.724) for the short interest sample and 0.567 (0.813) for the insider trading sample. The mean (median) *TEMP/BI* for the short interest sample is 0.097 (0.013) and for

¹⁵ Transaction codes must be “S” or “P” and relationship codes are “CB,” “D,” “DO,” “H,” “OD,” “VC,” “AV,” “CEO,” “CFO,” “CI,” “CO,” “CT,” “EVP,” “O,” “OB,” “OP,” “OS,” “OT,” “OX,” “P,” “S,” “SVP,” and “VP.”

¹⁶ We explore estimating a regression separately for loss firms using the following technique to rescale book income. We add the absolute magnitude of the largest loss observation *c* in each year plus 1 to the reported book income; i.e., $BI^* = BI + c + 1$. We use BI^* to form new variables TI/BI^* , $TEMP/BI^*$, and $PERM/BI^*$ and then use their rank values in the regressions. There is no predictability for earnings growth or for returns, similar to prior literature finding an absence of the accrual anomaly and tax momentum anomaly for the sample of loss firms. Also see Hayn (1995).

TABLE 1
Sample Selection
(NYSE and NASDAQ Firms from 1988 to 2009)

Panel A: Short Interest Sample Selection Process

Fifth month short interest after firms' fiscal year-ends	118,129
Deleting negative book income	(27,800)
Missing Compustat and CRSP data	(12,009)
Number of observations for analysis	78,320
Number of unique firms	5,819
Stock Exchange	
NYSE	40,746
NASDAQ	37,574
SFAS No. 109 Period	
Pre	16,849
Post	61,471

Panel B: Insider Trading Sample Selection Process

Insider trading within 20 days annual earnings announcement windows	70,147
Deleting negative book income	(12,184)
Missing Compustat and CRSP data	(10,672)
Number of observations for analysis	47,291
Number of unique firms	4,761
Stock Exchange	
NYSE	22,574
NASDAQ	24,717
SFAS No. 109 Period	
Pre	9,723
Post	37,568

the insider trading sample is 0.079 (0.012). The mean *PERM/BI* for the short interest (insider trading) sample is 0.201 (0.297) and its median is 0.070 (0.076).¹⁷ These variables thus reflect skewness, which in part motivates our use of ranks as discussed below.

Summary statistics for the control variables are reported in Table 2, Panel B. Both samples are represented by fairly large firms with the insider trading sample (median market cap \$839 million) being somewhat larger than the short interest sample (median market cap \$579 million). Both samples are generally widely followed with a median of 5.5 analysts for the short interest sample and 6 for the insider trading sample, and are predominantly held by large institutions with median holdings of 57.8 percent in the short interest sample and 56.5 percent in the insider trading sample. Mean and median percent operating accruals (*%OAcc*) are negative whereas they are positive for abnormal accruals (*AbnAcc*) and approximate about 1 percent of average total assets, where the

¹⁷ Using unwinsorized data, $[1 - \text{the mean of } TI/BI] = [\text{mean of } TEMP/BI + \text{mean of } PERM/BI]$.

TABLE 2

Descriptive Statistics and Variable Definitions

Panel A: Main Variables

Short Interest Sample					Insider Trading Sample				
	Mean	Q1	Median	Q3		Mean	Q1	Median	Q3
SI%	3.0247	0.1979	1.1074	3.6665	IT%	0.7217	−0.0002	0.1228	0.5023
TI/BI	0.6590	0.1144	0.7235	1.0148	TI/BI	0.5674	0.2855	0.8132	1.0654
TEMP/BI	0.0968	−0.0722	0.0132	0.2656	TEMP/BI	0.0793	−0.0864	0.0119	0.2340
PERM/BI	0.2013	−0.0890	0.0703	0.4758	PERM/BI	0.2966	−0.0642	0.0762	0.4577
n = 78,320					n = 47,291				

Panel B: Control Variables

	Short Interest Sample		Insider Trading Sample	
	Mean	Median	Mean	Median
%OAcc	−1.751	−0.722	−1.574	−0.655
AbnAcc	0.013	0.010	0.012	0.009
AF	7.933	5.5	8.268	6
BM	0.525	0.439	0.473	0.399
Div	0.012	0.000	NA	NA
E	0.079	0.063	NA	NA
E/P	0.068	0.057	NA	NA
Exchg	0.518	1	NA	NA
F-Score	5.100	5	5.182	5
IO(%)	0.560	0.578	0.548	0.565
Leverage	0.194	0.150	NA	NA
Litigation	NA	NA	0.039	0
MOM	0.228	0.097	0.257	0.150
Ret	0.018	0.009	NA	NA
RDCAPEX	0.161	0.086	0.125	0.090
Size (\$\$M)	4310.291	579.498	5183.861	838.670
STD	0.122	0.102	0.114	0.097
TaxMom	0.002	0.001	0.007	0.003
Turnover	0.016	0.009	NA	NA
n	78,320		47,291	

Additional variable definitions used in tests: *X* refers to quintile ranks of *AbnAcc* and *%OAcc* and discrete values of *F-Score*. *G* is the earnings growth is the difference between earnings in year *t* and earnings in year *t* − 1, scaled by total asset in year *t* (Compustat). Guenther (2011) Set of Variables are (1) firm age; (2) highSPI1 = 1 if special items/average total assets > 0.07; (3) lowSPI1 = 1 if special items/average total assets < −0.07; (4) highSPI2 = 1 if non-operating income/average total assets > 0.1; (5) lowSPI2 = 1 if non-operating income/average total assets < −0.1; (6) highSPI3 = 1 if gain or loss reported on the income statement/average total asset > 0; (7) lowSPI3 = 1 if gain or loss reported on the income statement/average total asset < 0; (8) highSPI4 = 1 if gain or loss reported on the statement of cash flows/average total assets > 0.07; (9) lowSPI4 = 1 if gain or loss reported on the statement of cash flows/average total assets < −0.07 (Compustat).

Variable Definitions:
TI/BI = taxable income * (1 − *tx*)/Book Income. Book Income is income before extraordinary items. The current portion of the income tax expense is the sum of current federal and foreign income taxes, or, when either amounts is missing, the difference between total income tax expense and the deferred portion of the income tax expense (Compustat). *R_TI/BI* is the quintile rank of *TI/BI*;

(continued on next page)

TABLE 2 (continued)

$TEMP/BI = \{(DTE/tx) * (1 - tx)\} / \text{Book Income}$; DTE = deferred tax expense, the sum of deferred federal and foreign tax expense, or, when either of these amounts is missing, as the deferred portion of the income tax expense (Compustat). R_TEMP/BI is the quintile rank of $TEMP/BI$;
 $PERM/BI = ([\text{Book Income} - \{\text{Taxable Income} * (1 - tx)\}] - TEMP) / \text{Book Income}$ (Compustat). R_PERM/BI is the quintile rank of $PERM/BI$;
 $SI\%$ = short interest position four months after the fiscal year-end (as reported on NASDAQ or NYSE monthly short interest files) divided by the number of shares outstanding in the same month as reported by CRSP, then multiplied by 100 to express as a percentage (NASDAQ and NYSE);
 $IT\%$ = number of shares sold net of purchases by insiders in the 20-trading days after earnings announcements/number of shares outstanding at fiscal year-end, then multiplied by 100 (Thomson Reuters Insider Trading database);
 $\%OAcc$ ($High\%OAcc$) = difference between net income and cash from operation, scaled by the absolute value of net income (Compustat). (1 if the firm-year ranks among the highest percent accruals quintile, 0 otherwise);
 $AbnAcc$ ($HighAbnAcc$) = residual from cross-sectional modified Jones model (Compustat). (1 if the firm-year ranks among the highest abnormal accruals quintile, 0 otherwise);
 AF = number of analysts following a firm (I/B/E/S). $\ln AF$ is the log of $1 + AF$;
 BM = book-to-market ratio: book value of common equity/Size (Compustat). $\ln BM$ is the log of BM ;
 Div = dividends/total asset (Compustat);
 E = earnings before extraordinary items/total assets (Compustat);
 E/P = E is earnings before extraordinary items, and P is number of shares outstanding $\times$ price per share at fiscal year-end (Compustat);
 $Exchg$ = 1 for NYSE firms, 0 for NASDAQ firms (Compustat);
 $F\text{-Score}$ ($LowF$) = sum of nine binary indicators: $\sum$ (positive return of assets (ROA), positive CFO, positive change in ROA, negative accruals, negative change in leverage (or no long-term debt), positive change in current ratio, no issuance of common equity, positive change in gross margin, positive change in asset turnover) (Compustat). (1 if a firm-year $F\text{-Score}$ is between 0 and 3, 0 otherwise);
 $IO(\%)$ = percent of shares owned by institutions at end of the most recent calendar quarter (Thomson Reuters 13F database). $\ln IO$ is the log of IO ;
 $Leverage$ = total long-term debt/total assets (Compustat);
 $Litigation$ = 1 for SIC industries 2833, 2836, 3570, 3577, 3600–3674, 5200–5961, and 7370–7374, 0 otherwise (Compustat);
 MOM (momentum) = the compounded monthly return for the window (–12,–2) from the short position report month (CRSP);
 Ret = monthly raw returns (CRSP);
 $RDCAPEX$ = sum of R&D expenses and capital expenditures total sales (Compustat);
 $Size$ (\$M) = number of shares outstanding multiplied by the fiscal year-end price (Compustat). $\ln Size$ is the log of $Size$;
 STD = standard deviation of the residuals for daily market adjusted returns over a one-year window ending one month prior to the month of reported short position (CRSP);
 $TaxMom$ = difference between tax expense per share in year t and year $t-1$ scaled by assets per share in year $t-1$ (Compustat); and
 $Turnover$ = monthly stock trading volume in millions of dollars divided by $Size$ (CRSP and Compustat).

latter is consistent with the average firm managing earnings upwards. $F\text{-Score}$, which ranges from 0 to 1, averages about 5 in both samples, which indicates that our samples are representative of the general population of firms where the mid-range of $F\text{-Score}$ lies between 4 and 6. The mean (median) book-to-market ratio is somewhat higher in the short interest sample, 0.53 (0.44), *vis-à-vis* the insider trading sample, 0.47 (0.40), suggesting a greater presence of value firms in the short interest sample and higher growth opportunities in the insider trading sample. All financial statement variables are winsorized at the 1st and 99th percentiles.

IV. EARNINGS AND STOCK RETURN PREDICTABILITY

Predicting Earnings Growth

We initially test whether BTD variables contain information about future earnings performance in our sample before evaluating whether investors understand this information and value the firm accordingly. We regress earnings growth G on TI/BI , accruals, and other determinants of earnings

growth in the following regression:

$$G_t = a_0 + a_1 R\_TI/BI_{t-1} + a_2 X_{t-1} + a_3 TaxMom_{t-1} + a_4 BM_{t-1} + a_5 E/P_{t-1} + a_6 E_{t-1} + a_7 Div_{t-1} + a_8 RDCAPEX_{t-1} + a_9 LnSize_{t-1} + a_{10} Guenther_{t-1} + \varepsilon_t. \quad (2)$$

The dependent variable G is measured as the annual change in earnings scaled by the lagged total assets. The key explanatory variable, R_TI/BI , is the quintile rank of TI/BI . X represents the key control variables and contains the set of accrual-related predictor variables, namely abnormal accrual rank R_AbnAcc , percent operating accrual rank $R\_ \%OAcc$, and $F-Score$ to test for incremental predictability of TI/BI . Following Lev and Nissim (2004), we use quintile ranks sorted by industry and year instead of the continuous values for TI/BI because the earnings growth relation with BTD variables may be nonlinear. A positive a_1 coefficient implies that higher TI/BI ranks predict higher future earnings.

We control for other determinants of earnings growth, including size, book-to-market and earnings-price ratios, prior earnings level, dividends, and $RDCAPEX$ (sum of R&D and capital expenditures) from Lev and Nissim (2004), tax momentum from Thomas and Zhang (2011), and persistence-related variables that Guenther (2011) links to BTDs, as detailed in Table 2.¹⁸

An important extension of our tests is that we separate the effects of temporary versus permanent components of TI/BI , $TEMP/BI$, and $PERM/BI$, respectively, for the predictability of earnings growth and use their quintiles rank as shown in regression (3):

$$G_t = a_0 + a_1 R\_TEMP/BI_{t-1} + a_2 R\_PERM/BI_{t-1} + a_3 X_{t-1} + a_4 TaxMom_{t-1} + a_5 BM_{t-1} + a_6 E/P_{t-1} + a_7 E_{t-1} + a_8 Div_{t-1} + a_9 RDCAPEX_{t-1} + a_{10} LnSize_{t-1} + a_{11} Guenther_{t-1} + \varepsilon_t. \quad (3)$$

The key control variable X and all other control variables are defined as before.

For models (2) and (3), we run pooled OLS regressions with year fixed effects and estimate standard errors clustered by firm (Petersen 2009; Gow, Ormazabal, and Taylor 2010). Because of important changes in the accounting for income taxes under SFAS No. 109, we include an interaction variable between the BTD variables and an indicator variable $Post109$ for 1993 onward.¹⁹

Table 3, Panel A reports results from model (2). Consistent with Lev and Nissim (2004), R_TI/BI is significantly positive at the $p < 0.01$ level ($t = 10.42$), indicating that a higher TI/BI ratio predicts higher future earnings growth. Economically, one-year-ahead earnings growth increases 50 percent from the lowest quintile to the highest quintile of R_TI/BI , simultaneously controlling for $AbnAcc$, $\%OAcc$, and $F-Score$.²⁰ Our contribution is that we show that TI/BI incrementally predicts earnings growth beyond other accounting-based return predictors ($AbnAcc$, $\%OAcc$, and $F-Score$), $TaxMom$, and the Guenther (2011) persistence-related variables. The results also show that the $Post109$ interaction variable is insignificant, which suggests that the relation between earnings growth and TI/BI is not different pre- and post-109.

Turning to the distinction between temporary and permanent BTDs, Table 3, Panel B shows that R_TEMP/BI is significantly negative, whereas R_PERM/BI is insignificant, where larger $TEMP/BI$ represents smaller TI/BI . The absolute magnitude of the coefficient on $TEMP/BI$ clearly dominates that for $PERM/BI$. The results also show that the interaction variables with $Post109$ are

¹⁸ Hanlon (2005) finds that firms with large temporary BTDs have lower persistence of earnings and accruals.

¹⁹ During our pre-109 sample, accounting standards for income taxes were in flux and two different standards (APBO No. 11, Accounting Principles Board [APB 1967], and SFAS No. 96 (Financial Accounting Standards Board [1987])) were permitted.

²⁰ 50 percent = $0.125 * (5 - 1)$ from the coefficients in Table 3, Panel A.

TABLE 3
Multivariate Analysis of One-Year-Ahead Earnings Growth and Book-Tax Difference Measures

Panel A: *TI/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		−0.174***	−13.69	−0.155***	−13.02
<i>R</i> <i>TI/BI</i>	+	0.125***	10.42	0.127***	8.77
<i>R</i> <i>TI/BI</i> * <i>Post109</i>				0.006	1.63
<i>Post109</i>				0.078	1.22
<i>TaxMom</i>		1.384**	1.99	1.511**	2.16
<i>R</i> <i>AbnAcc</i> , <i>R</i> <i>%OAcc</i> , <i>F-Score</i> , and <i>Guenther</i> Variables		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year * Firm	
n		78,320		78,320	
Adjusted R ²		16.81%		16.76%	

Panel B: *TEMP/BI* and *PERM/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		−0.754***	−5.95	−0.428***	−3.54
<i>R</i> <i>TEMP/BI</i>	−	−0.112***	−10.07	−0.204***	−9.47
<i>R</i> <i>PERM/BI</i>	?	−0.009	−0.75	−0.010	−1.09
<i>R</i> <i>TEMP/BI</i> * <i>Post109</i>	−			−0.074***	−2.82
<i>R</i> <i>PERM/BI</i> * <i>Post109</i>	?			−0.002	−1.49
<i>Post109</i>				0.184*	1.93
<i>TaxMom</i>		1.167*	1.67	1.328*	1.89
<i>R</i> <i>AbnAcc</i> , <i>R</i> <i>%OAcc</i> , <i>F-Score</i> , and <i>Guenther</i> Variables		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year * Firm	
n		78,320		78,320	
Adjusted R ²		18.01%		17.89%	

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels (one-tailed for signed predictions; two-tailed for all else), respectively.

Table 3 presents the results of models (2) and (3), *TI/BI*, *TEMP/BI*, and *PERM/BI* predictability of future earnings growth. The dependent variable is the one-year-ahead earnings growth. *R* *TI/BI*, *R* *TEMP/BI*, and *R* *PERM/BI* are the industry-year quintile ranks of *TI/BI*, *TEMP/BI*, and *PERM/BI*. *R* *AbnAcc*, *R* *%OAcc*, and *F-Score* are simultaneously controlled. Other Controls include *LnSize*, *BM*, *E/P*, *E*, *Div*, *RDCAPEX*, and *Guenther* (2011) variables. The sample consists of NYSE and NASDAQ firms from 1988 to 2009. Variable definitions are in Table 2.

significantly negative only for $TEMP/BI$.²¹ Hence, the predictive power of TI/BI derives primarily from $TEMP/BI$ rather than $PERM/BI$ and the predictive power of $TEMP/BI$ for future earnings growth is especially strong under SFAS No. 109. Economically, R_TEMP/BI gains an additional 26.6 percent predictability of earnings growth in the post-109 period controlling for $AbnAcc$, $\%OAcc$, $F-Score$, $TaxMom$, and the Guenther (2011) variables.²²

Trading Strategy Profits from Characteristics-Adjusted Hedge Returns

Next, we investigate whether TI/BI and $TEMP/BI$ predict stock returns in two ways. First, we compute characteristics-adjusted hedge portfolio returns and, second, we estimate Fama-MacBeth panel regressions.

We calculate trading strategy profits to long-short portfolios that are sorted based on TI/BI and $TEMP/BI$ as estimates of the economic significance of the TI/BI and $TEMP/BI$ anomalies. We form quintile portfolios monthly based on the industry-ranked values of TI/BI of the most recent available fiscal year, allowing for a four-month lag between fiscal year-end and the portfolio formation month. We estimate equal-weighted monthly excess returns in the top and bottom quintiles using the portfolio characteristics-adjusted approach in Daniel, Grinblatt, Titman, and Wermers (1997) to control for firm size, BM , and momentum (MOM). The excess returns for firms in the long and short quintiles are obtained by subtracting returns to benchmark portfolios in the same size, BM , and MOM quintiles that the stock belongs to from the firm's returns. We form benchmark portfolios by sequential sorts, first by size quintiles, then within each size quintile further sort into BM quintiles, and finally sort within each of the 25 size and BM portfolios into quintiles based on firms' past 12-month returns (skipping the most recent month). This yields 125 benchmark portfolios, and the benchmark portfolio returns are calculated by equally weighting returns of the individual firm members in the portfolios.

The TI/BI trading strategy is long in the highest TI/BI quintile and shorts the lowest TI/BI quintile, so the hedge return is quintile 5 excess returns (R_H) minus quintile 1 excess returns (R_L). The t-statistics for the hedge returns are based on the time-series of the monthly mean hedge portfolio excess returns. We also compute trading strategy hedge profits based on sorting singly by $AbnAcc$, $\%OAcc$, $F-Score$, and $TaxMom$ for comparison (untabulated). As in prior literature, the $AbnAcc$ ($\%OAcc$) hedge profits are earned by going long in the lowest $AbnAcc$ ($\%OAcc$) portfolio and shorting the highest $AbnAcc$ ($\%OAcc$) portfolio. Conversely, the $F-Score$ ($TaxMom$) hedge profits are earned by going long in the highest $F-Score$ ($TaxMom$) portfolio and shorting the lowest $F-Score$ ($TaxMom$) portfolio.

Firms are then double-sorted into TI/BI quintiles and each of the four alternative accounting predictor ($AbnAcc$, $\%OAcc$, $F-Score$, and $TaxMom$) quintiles. For example, the hedge profits are calculated as the excess returns for the portfolio of firms that belong jointly to the bottom $AbnAcc$ ($\%OAcc$) quintile and the top TI/BI quintile minus the excess returns for the portfolio of firms that belong jointly to the top $AbnAcc$ ($\%OAcc$) quintile and the bottom TI/BI quintile. We repeat these hedge profit calculations for $TEMP/BI$, where the hedge strategy is long in the lowest $TEMP/BI$ quintile and short in the highest $TEMP/BI$ quintile. Given the weak predictability of $PERM/BI$, we do not tabulate the results for $PERM/BI$.

²¹ An untabulated F-test (p-value < 0.001) shows the sum of coefficients on R_TEMP/BI and $R\_TEMP/BI * Post109$ (−0.278) is significantly more negative than that for R_PERM/BI and $R\_PERM/BI * Post109$ (−0.012).

²² We calculate this from the coefficients in Table 3, Panel B. For $R\_TEMP/BI = 1$, the coefficient in the pre-109 period is $(-0.204 * 1) - (0.074 * 1 * 0) = -0.204$, in the post-109 period it is $(-0.204 * 1) - (0.074 * 1 * 1) = -0.278$. Therefore, the percentage change is $(\text{post-109 coefficient} - \text{pre-109 coefficient})/\text{post-109 coefficient}$, which is $-0.074/-0.278 = 26.62$ percent.

TABLE 4
Monthly Hedge Returns, Return Asymmetries, and Book-Tax Difference Measures

Panel A: Hedge Portfolio on *TI/BI* or *TEMP/BI*

<i>TI/BI</i>				<i>TEMP/BI</i>			
	Full Period	Pre-109 Period	Post-109 Period		Full Period	Pre-109 Period	Post-109 Period
<i>L</i> <i>TI/BI</i>	-0.0057	-0.0061	-0.0051	<i>L</i> <i>TEMP/BI</i>	0.0015	0.0007	0.0017
<i>H</i> <i>TI/BI</i>	0.0015	0.0014	0.0013	<i>H</i> <i>TEMP/BI</i>	-0.0032	-0.0025	-0.0038
Hedge Return	0.0072 (8.11)***	0.0075 (7.16)***	0.0064 (6.36)***	Hedge Return	0.0047 (7.81)***	0.0032 (2.69)***	0.0055 (5.21)***
Return Asymmetry	0.0042 (4.05)***	0.0047 (4.49)***	0.0038 (3.78)***	Return Asymmetry	0.0017 (2.74)***	0.0018 (1.53)	0.0021 (1.99)**

Panel B: Hedge Portfolio on Joint *TI/BI* or *TEMP/BI* and *AbnAcc*

<i>AbnAcc and TI/BI</i>				<i>AbnAcc and TEMP/BI</i>			
	Full Period	Pre-109 Period	Post-109 Period		Full Period	Pre-109 Period	Post-109 Period
<i>L</i> <i>AbnAcc and</i> <i>H</i> <i>TI/BI</i>	0.0044	0.0042	0.0046	<i>L</i> <i>AbnAcc and</i> <i>L</i> <i>TEMP/BI</i>	0.0042	0.0034	0.0045
<i>H</i> <i>AbnAcc and</i> <i>L</i> <i>TI/BI</i>	-0.0115	-0.0123	-0.0105	<i>H</i> <i>AbnAcc and</i> <i>H</i> <i>TEMP/BI</i>	-0.0092	-0.0080	-0.0101
Hedge Return	0.0159 (5.12)***	0.0165 (4.33)***	0.0151 (5.14)***	Hedge Return	0.0134 (7.99)***	0.0114 (3.59)***	0.0146 (7.93)***
Return Asymmetry	0.0071 (3.58)***	0.0081 (2.19)**	0.0059 (2.09)**	Return Asymmetry	0.0050 (2.98)***	0.0046 (1.45)	0.0056 (3.13)***

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed). The time-series averages of the monthly hedge returns and return asymmetries based on *TI/BI* (*TEMP/BI*) are reported along with their t-statistics over the periods shown. The t-statistics reported in parentheses are calculated based on a time-series of 260 month portfolio abnormal stock returns (Fama and MacBeth 1973) in the full sample, 72 month in the pre-109 period (May 1988 to April 1994), and 188 month in the post-109 period (May 1994 to December 2009). Benchmark portfolio returns are calculated as equal-weighted size-BM-momentum adjusted based on Daniel et al. (1997). Hedge returns are the difference between long and short positions. Return asymmetries are the absolute value of short plus long positions. Returns in italics are significant at the 1 percent level. Variable definitions are in Table 2.

Our hedge portfolio analysis involves monthly rebalancing over the 260 months in the full sample period after allowing for a four-month lag and 72 (188) months in the pre- (post-) 109 sub-period.²³ Table 4, Panel A shows that over the full sample period the hedge profit for *TI/BI* is 72 basis points per month ($t=8.11$), or almost 9 percent annualized, which is economically significant. The hedge returns for a *TEMP/BI* strategy are also statistically and economically significant, and larger under post-109 at 55 basis points per month ($t=5.21$).

²³ SFAS No. 109 was effective for fiscal years beginning after mid-December 1992. For our pre-109 sample period, we measure hedge returns for the fifth month after the fiscal year-end, beginning with May 1988 and ending with April 1994; for the post-109 period we begin with May 1994 and end with December 2009.

Untabulated results show that the single-sort trading strategies for the four alternative predictors are statistically significant and similar in magnitudes to those reported in prior studies. For example, for the full sample period 1988 to 2009 a strategy of going long in the lowest *AbnAcc* quintile and short in the highest *AbnAcc* quintile yields positive average monthly hedge portfolio excess returns of 1.07 percent ($t = 7.43$). As a basis for comparison, our *TI/BI* hedge strategy of 0.72 percent per month is approximately 67 percent as large as the accruals anomaly.

More relevant for our study, Table 4, Panel B reports the joint sort hedge profits for either *TI/BI* or *TEMP/BI* with abnormal accruals, one of the alternative accounting predictors. A joint strategy sorted on *AbnAcc* and *TI/BI* earns a substantial 1.59 percent per month ($t = 5.12$). This amount is equivalent to 89 percent of the summed hedge profits ($0.72 + 1.07 = 1.79$ percent) that could be earned if there were no overlap in the rankings of firms sorted separately by *TI/BI* and by *AbnAcc*. These results, which are qualitatively similar in the pre- and post-109 periods, indicate that the *TI/BI* anomaly is largely distinct and incremental to the accruals anomaly by 52 (i.e., $\text{Difference} = 159 - 107 = 52$ basis points per month or 6.4 percent year, $t = 2.29$, and $p < 0.05$ level).²⁴ This method of estimating incremental hedge profits to a *TI/BI* or *TEMP/BI* strategy over another predictor is conservative because any correlated effects of *TI/BI* or *TEMP/BI* with the *AbnAcc* are ascribed to *AbnAcc* rather than to *TI/BI* or *TEMP/BI*.

The limited overlap between *AbnAcc* and *TI/BI* suggests that abnormal accruals is an insufficient proxy for earnings management. Since *TI/BI* also arises from an accruals process, the results suggest that it is an incremental proxy for earnings management as well. Consistent with Weber (2009), we find in untabulated results that joint *TI/BI* and *AbnAcc* hedge portfolio returns are greater in the presence of no or low analyst following relative to high analyst following (1.74 versus 1.28 percent per month), and the results are qualitatively the same in both the pre- and post-109 periods.

In the post-109 period, the joint strategy for *TEMP/BI* with *AbnAcc* (Table 4, Panel B) yields a monthly return spread of 134 basis points ($t = 7.99$), which significantly exceeds the hedge returns of 103 basis points from sorting on *AbnAcc* alone by 31 basis points per month. Consistently, the untabulated monthly hedge returns of 87, 90, and 153 basis points from the joint strategy for *TEMP/BI* with *%OAcc*, *F-Score*, or *TaxMom*, respectively, are significantly higher than the untabulated hedge returns from sorting on *%OAcc*, *F-Score*, or *TaxMom* alone, which are 56, 51, and 83 basis points, respectively. Hedge returns are also greater for low or no analyst following *vis-à-vis* high analyst following (untabulated).²⁵

Figures 1 and 2, respectively, show histograms of average monthly incremental hedge returns aggregated to an annual basis for *TI/BI* over the 22 years of our full sample and for *TEMP/BI* over the 16 years in our post-109 period. The hedge returns are incremental beyond each of the three accounting-based return predictor variables, *AbnAcc*, *%OAcc*, and *F-Score*. For the 22 annual hedge returns for *TI/BI* incremental to *AbnAcc*, 17, or 77 percent are positive; for the *TI/BI* hedge returns incremental to *%OAcc*, 77 percent are positive, and for *F-Score*, 86 percent are positive. With respect to *TEMP/BI*, hedge returns are positive and incremental to *AbnAcc* in 13 of the 16 years or 81 percent of the cases in the post-109 period, and 75 percent and 69 percent, respectively,

²⁴ The sub-period hedge returns are as follows: for *TI/BI*, 0.75 percent per month pre- and 0.64 percent per month post-109; for *AbnAcc*, 1.14 and 1.03 percent per month in the pre- and post-109 periods, respectively; and for the joint *AbnAcc* and *TI/BI* portfolio, 1.65 and 1.51 percent per month in the pre- and post-109 periods, respectively.

²⁵ A joint *%OAcc* (*F-Score*; *TaxMom*) and *TI/BI* (*TEMP/BI*) strategy does not change our inferences. Regarding *PERM/BI*, untabulated results indicate small and insignificant excess monthly returns (0.19 percent, $t = 0.94$). A joint *AbnAcc* and *PERM/BI* hedge portfolio earns only 0.70 percent, which is less than the 1.03 percent monthly return the accrual anomaly-based hedge earns by itself. Hence, including *PERM/BI* has the effect of lowering the joint hedge return and thus the *PERM/BI* effect is not incremental to the accruals anomaly. A joint *%OAcc* (*F-Score*; *TaxMom*) and *PERM/BI* strategy does not change our inferences.

FIGURE 1
Average Incremental Monthly Hedge Returns (*TI/BI*)

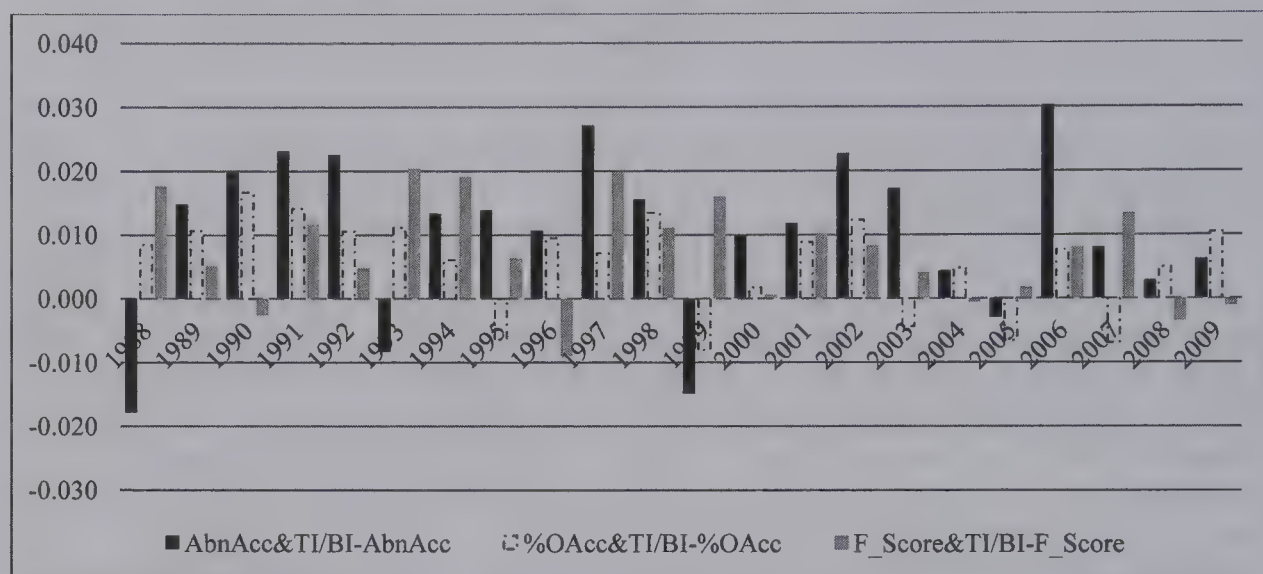


Figure 1 represents the annual incremental hedge returns (vertical axis). The horizontal axis represents years from 1988 to 2009 for *TI/BI*. In Figure 1, the monthly incremental hedge returns are calculated as the monthly hedge return from the joint strategy sorted on the accounting-based predictor (*AbnAcc*, *%OAcc*, or *F-Score*) and *TI/BI* minus the monthly hedge return from the strategy sorted on the accounting-based predictor (*AbnAcc*, *%OAcc*, or *F-Score*) alone. We then aggregate the incremental monthly hedge returns to an annual basis. The incremental hedge returns are shown as “*AbnAcc* (*%OAcc* or *F-Score*) & *TI/BI* – *AbnAcc* (*%OAcc* or *F-Score*)” in Figure 1.

are incrementally positive beyond *%OAcc* and *F-Score*. Hence, the hedge profits are not limited to a small number of years but rather span a significant majority of the sample periods examined. We suggest a limited attention explanation for why the *TI/BI* and *TEMP/BI* anomalies exist in the Introduction and in the Conclusion sections. One explanation as to why the anomaly persists is that during our sample period there are mandated changes on accounting policies, including the accounting for income taxes, affecting book income as well as changes in tax policies affecting taxable income, along with non-disclosure of taxable income. Learning by investors is difficult with a changing landscape for interpreting the information contained in *BTDs* about future fundamentals, and so *BTD* mispricing continues to occur.

Regression Analyses Predicting Stock Returns

The Fama-MacBeth cross-sectional regressions are performed monthly between May 1988 and December 2009. Following Hirshleifer et al. (2011), we allow a minimum four-month lag between the fiscal year-end, to measure the accounting predictor variables, and the start of the predictive return month, and a one-month lag between the measurement of the standard risk control proxies and the return month. Thus, the panel regressions roll forward monthly for 12 months before the annual accounting predictor variables are updated. For example, raw returns, *Ret*, for April 2001 is regressed on *R_TI/BI* measured for the fiscal year ending December 2000, along with the set of control variables that are also for the fiscal year ending December 2000, and the standard asset pricing risk proxies, *LnSize*, *BM*, and 12-month stock return momentum, are updated monthly.

FIGURE 2
Average Incremental Monthly Hedge Returns (TEMP/BI)

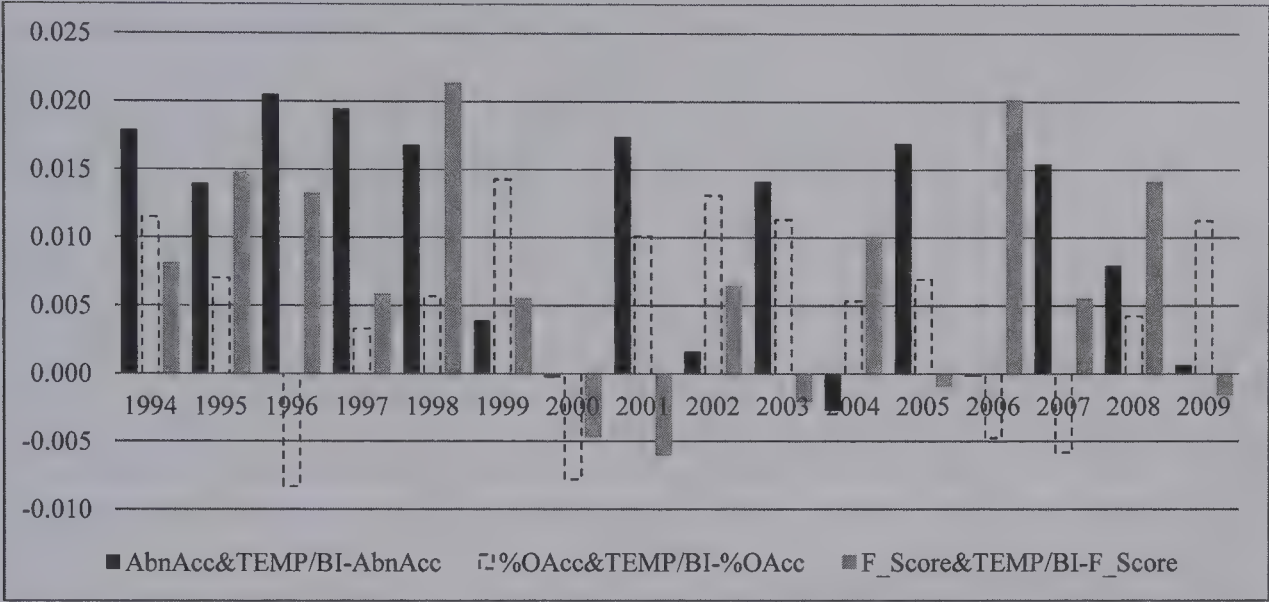


Figure 2 represents the annual incremental hedge returns (vertical axis). The horizontal axis represents years from 1994 to 2009 for *TEMP/BI* (post-109 in Figure 2). In Figure 2, the monthly incremental hedge returns are calculated as the monthly hedge return from the joint strategy sorted on the accounting-based predictor (*AbnAcc*, *%OAcc*, or *F-Score*) and *TEMP/BI* minus the monthly hedge return from the strategy sorted on the accounting-based predictor (*AbnAcc*, *%OAcc*, or *F-Score*) alone. We then aggregate the incremental monthly hedge returns to an annual basis. The incremental hedge returns are shown as “*AbnAcc* (*%OAcc* or *F-score*) & *TEMP/BI* – *AbnAcc* (*%OAcc* or *F-score*)” in Figure 2.

Since the dependent variable is monthly returns, there are no overlapping returns across the monthly regressions. We report the time-series average of the monthly coefficient estimates, and use the time-series standard deviations of the coefficients as standard errors to obtain the Fama-MacBeth t-statistics. High quintile R_TI/BI and low quintile R_TEMP/BI are expected to predict positive future returns. The regressions are:

$$Ret_t = a_0 + a_1R\_TI/BI_{t-1} + a_2X_{t-1} + a_3TaxMom_{t-1} + a_4Guenther_{t-1} + Asset Pricing Risk Controls + \epsilon_t. \tag{4}$$

$$Ret_t = a_0 + a_1R\_TEMP/BI_{t-1} + a_2R\_PERM/BI_{t-1} + a_3X_{t-1} + a_4TaxMom_{t-1} + a_5Guenther_{t-1} + Asset Pricing Risk Controls + \epsilon_t. \tag{5}$$

TI/BI Anomaly

Table 5, Panel A presents the results of stock return predictability by *TI/BI* from Fama-MacBeth regressions in model (4). Results are shown for the full sample period and the pre- and post-109 sub-periods. The results for *TEMP/BI* and *PERM/BI* are in Table 5, Panel B.

R_TI/BI is significantly positive at the $p < 0.01$ level in all three columns of Table 5, Panel A, so *TI/BI* strongly predicts abnormal stock returns incrementally to other accounting predictors and controls, and it does so both pre- and post-109. The proportions of months with positive coefficients

TABLE 5

Fama-MacBeth Monthly Stock Return Regressions on Book-Tax Difference Measures

Panel A: *TI/BI*

Variables	Prediction	Full Period		Pre-109 Period		Post-109 Period	
		Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Intercept		3.136***	6.20	3.182***	4.03	3.165***	4.56
<i>R_TI/BI</i>	+	0.038***	2.53	0.044***	2.64	0.035***	2.57
<i>TaxMom</i>		1.247***	7.49	1.991***	8.11	1.366***	8.48
<i>R_AbnAcc</i> , <i>R_%OAcc</i> , <i>F-Score</i> , and <i>Guenther</i> Variables		Yes		Yes		Yes	
Asset Pricing Risk Controls		Yes		Yes		Yes	
Regression		FM		FM		FM	
Standard Errors		FM		FM		FM	
n (Months)		260		72		188	
Adjusted R ²		6.56%		5.69%		6.46%	
# of Months: Positive Returns from <i>R_TI/BI</i>		146**		45**		104***	

Panel B: *TEMP/BI* and *PERM/BI*

Variables	Prediction	Full Period		Pre-109 Period		Post-109 Period	
		Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Intercept		2.998***	6.07	3.606***	4.66	2.785***	4.56
<i>R_TEMP/BI</i>	−	−0.048***	−3.25	−0.053	−1.39	−0.037***	−3.57
<i>R_PERM/BI</i>	?	−0.008	−0.52	−0.002	−0.80	−0.008	−0.47
<i>TaxMom</i>		1.763***	7.48	1.783***	7.65	1.747***	7.15
<i>R_AbnAcc</i> , <i>R_%OAcc</i> , <i>F-Score</i> , and <i>Guenther</i> Variables		Yes		Yes		Yes	
Asset Pricing Risk Controls		Yes		Yes		Yes	
Regression		FM		FM		FM	
Standard Errors		FM		FM		FM	
n (Months)		260		72		188	
Adjusted R ²		6.91%		5.89%		6.77%	
# of Months: Negative Returns from <i>R_TEMP/BI</i>		142**		32		113***	

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels (one-tailed for signed predictions; two-tailed for all else), respectively.

This table presents the results of models (4) and (5), *TI/BI*, *TEMP/BI*, and *PERM/BI* predictability of future returns. The dependent variable is the monthly raw returns starting month 5 following the fiscal year-end. *R_TI/BI*, *R_TEMP/BI*, *R_PERM/BI* are the industry-year quintile ranks of *TI/BI*, *TEMP/BI*, and *PERM/BI*, respectively. *R_AbnAcc*, *R_%OAcc*, and *F-Score* are simultaneously controlled. Asset pricing risk controls are *LnSize*, *LnBM*, and *MOM*. The accounting annual variables are from the most recent fiscal year. Monthly average coefficients and Fama-MacBeth adjusted standard errors are reported. The full sample consists of NYSE and NASDAQ firms from May 1988 to December 2009 (260 months). The pre-109 period is from May 1988 to April 1994 (72 months). The post-109 period is from May 1994 to December 2009 (188 months).

Variable definitions are in Table 2.

are significantly greater than 50 percent at the $p < 0.05$ level or better. Also, the coefficients on *TaxMom* continue to predict stock returns, consistent with past studies.^{26,27}

Our contributions from this table are that we show incremental return predictability over other known return predictors, and that the evidence is unambiguous unlike in Lev and Nissim (2004). Moreover, these results confirm our earlier hedge portfolio results. Both our longer post-109 period, which covers 16 years instead of the seven years in the Lev and Nissim (2004) study that included the dot-com bubble, and the broader sample that includes non-December fiscal year-end firms likely contribute to the more conclusive evidence.

TEMP/BI Anomaly

Based on model (5), Table 5, Panel B shows that the *TI/BI* anomaly is derived from *TEMP/BI*, not *PERM/BI*. R_TEMP/BI is negative at the $p < 0.01$ level overall and in the post-109 period, and the proportion of months with negative coefficients on *TEMP/BI* is significantly greater than 50 percent ($p < 0.05$). The market appears to overvalue firms with high *TEMP/BI* and undervalues firms with low *TEMP/BI*. These findings are consistent with *TEMP/BI* being more able to capture managerial discretion and so is a less noisy proxy for both earnings and tax management than *PERM/BI*. A possible explanation for the return predictability of R_TEMP/BI being present only post-109 is the use of more forward-looking information under SFAS No. 109. However, we note that we have relatively fewer observations, and therefore a lower power test, for the pre- relative to the post-109 period. As before, *TaxMom* and the accounting-related predictors continue to predict returns. Our contribution over past studies is that we provide strong evidence of a *TEMP/BI* anomaly that is incremental to prior accounting-based anomalies under the current accounting standard for income taxes.

Together, the results support the conclusion that there is a *TI/BI* anomaly, and the source of the anomaly is primarily due to *TEMP/BI*. Temporary BTDs reflect less persistent accruals because of likely opportunistic managerial decisions enabled by the generally greater discretion that managers have in reporting book income relative to taxable income. *TEMP/BI*'s information regarding future earnings is therefore commonly not well understood by the general investor.

V. SHORT ARBITRAGE OF *TI/BI* (AND *TEMP/BI*) ANOMALY

Empirical Models

We turn now to our analysis of short selling and total BTD-related pricing anomalies. Short sellers are described as sophisticated investors (Diamond and Verrecchia 1987), and short arbitrage of overpriced stocks can be viewed as a response by these sophisticated investors to over-optimistic beliefs of other investors (Drake et al. 2011).

Short sellers are expected to use all available information, including sell-side financial analysts' information in their trades. It is possible that sell-side analysts reflect the market's information about the stock, with short sellers taking the opposite position from the rest of the market. If so, when market expectations are very positive, as reflected in very high forecasts, then stock price will be too high, and low *TI/BI* values would be especially good indicators of profit opportunities for short sellers, so we would expect to observe high short interest. Evidence of a relation between

²⁶ Thomas and Zhang (2011) show that tax expense momentum and *TI/BI* are only weakly related and that each is incremental to the other in predicting future stock returns.

²⁷ When we control *AbnAcc*, *%OAcc*, and *F-Score* simultaneously, *%OAcc* significantly predicts stock returns but not *AbnAcc* and *F-Score* because of multicollinearity. We also control *AbnAcc*, *%OAcc*, and *F-Score* individually in untabulated robust tests. Each of them continues to predict stock returns, consistent with past studies.

short interest and TI/BI as well as $TEMP/BI$ serve to triangulate our results and represent strong evidence that BTDs are mispriced by investors and are not a proxy for risk. The overpricing of BTDs may be from investors over-estimating future cash flows implied by BTDs or under-estimating the riskiness of future cash flows implied by BTDs.²⁸

Based on prior research, we estimate regression (6) to test the relation between short interest and TI/BI as to whether short sellers arbitrage the book-tax anomalies:

$$SI\%_t = a_0 + a_1R\_TI/BI_{t-1} + a_2HighAbnAcc_{t-1} + a_3High\%OAcc_{t-1} + a_4LowF_{t-1} + a_5TaxMom_{t-1} + a_6BM_t + a_7Turnover_t + a_8lnIO_t + a_9MOM_t + a_{10}STD_t + a_{11}Exchg_t + a_{12}lnAF_t + a_{13}Leverage_t + a_{14}lnSize_t + a_{15}Guenther_{t-1} + \varepsilon_t. \quad (6)$$

$SI\%$ is defined in Section III. If short sellers understand and use R_TI/BI as an indicator of future earnings, then short interest in a firm's shares should decrease with R_TI/BI . Hence, we expect that $a_1 < 0$ in regression (6). To test for short arbitrage of the BTD components, we replace R_TI/BI with R_TEMP/BI and R_PERM/BI and expect a positive coefficient on R_TEMP/BI .

As before, we simultaneously control for $TaxMom$ and the set of accrual-related predictors, $AbnAcc$, $\%OAcc$, and $F-Score$. Since short sellers accumulate positions in firms with high levels of accruals and low fundamentals (Hirshleifer et al. 2011; Dechow et al. 2001), we define $HighAbnAcc$ and $High\%OAcc$, respectively, as indicator variables that equal 1 if a firm-year is in the highest $AbnAcc$ or $\%OAcc$ quintile. Following Piotroski and So (2012), we define $LowF$ as an indicator variable that equals 1 if a firm-year $F-Score$ is between 0 and 3.²⁹

We control for other determinants and costs of short selling in the short interest regression based on previous research (Dechow et al. 2001; Jones and Lamont 2002; D'Avolio 2002; Asquith et al. 2005; Nagel 2005; Ali and Trombley 2006; Desai et al. 2006; Cohen, Diether, and Mallory 2007; Karpoff and Lou 2010; Hirshleifer et al. 2011). A key control variable is institutional ownership, since institutions are the main source of supply of loanable shares to short arbitrageurs. Hence, the level of institutional holdings, IO , is an important proxy for ease of short selling, and short selling is positively associated with IO . We obtain the institutional ownership data from the CDA/Spectrum database and, as in prior research, compute IO as the total number of a firm's shares held by institutions divided by the total number of shares outstanding at the end of a quarter, and then multiplied by 100 to express as a percentage. We match monthly $SI\%$ with IO of the latest available quarter, and use log of IO in the multivariate analysis.

Institutional ownership is an imperfect measure of the arbitrage costs. A better measure would be the rebate rate that shorts have to pay, but such data are unavailable. Therefore, we further control for ease of short selling using two other proxies, which are the number of analysts following a firm, AF , and a firm's financial leverage, $Leverage$. We compute AF and $Leverage$, respectively, as the log of $(1 + AF)$ and total long-term debt divided by total assets. In addition, short arbitrage is more active among liquid and volatile stocks. We use firm size and share turnover to proxy for liquidity. $Size$ is previously explained and $Turnover$ is monthly stock trading volume in millions of dollars divided by firm size. We measure volatility as the standard deviation of residuals for daily market-adjusted returns, STD , estimated over a one-year window ending one-month prior to the

²⁸ Note that higher risk tolerance of short sellers does not lead to greater shorting of firms with high TI/BI and low $TEMP/BI$. Consider the Sharpe CAPM world. More risk-tolerant investors move up the market line and so will weight their portfolio toward a greater proportion of the assets in positive net supply, that is the market portfolio, and less in the risk-free asset. Hence, they actually will not go short on the positive net supply of assets that have high risk just to bear more risk. They would go short on high-risk assets only if they think these assets are overpriced.

²⁹ Because the accrual-related predictors are highly correlated, we do not interpret the coefficients on these variables for whether there is short arbitrage of these predictors, and they are included only as controls.

month of the reported short position. High short interest is also expected to be associated with low *BM* firms and in stocks with low *MOM*. We also include a 0/1 indicator variable for stock exchange, where *Exchg* = 1 for NYSE, to further control for any factors associated with ease or constraints on short arbitrage that are not picked up by the included controls. For model (6), we run pooled OLS regressions with year fixed effects and estimate standard errors clustered by firm (Petersen 2009; Gow et al. 2010).³⁰

Multivariate Analyses of Short Interest on *TI/BI*, *TEMP/BI*, and *PERM/BI*

Table 6, Panel A presents the regression results of model (6) for short arbitrage of *TI/BI*. First, consistent with prior literature, *SI*, the dependent variable, is positively and significantly related to *Leverage*, *lnAF*, *lnIO*, *lnSize*, *Turnover*, and *STD* (untabulated). These results are consistent with greater short interest when the supply of loanable shares is more ample, among more liquid stocks, and among more volatile stocks. Short selling is also more active among low momentum and low book-to-market stocks and firms with high abnormal accruals.

With regard to *R_TI/BI*, as predicted the results in Table 6, Panel A reveal that its coefficients are negative and significant at the $p < 0.01$ level controlling for *AbnAcc*, *%OAcc*, *F-Score*, and *TaxMom*. On average, short interest increases 32.8 percent from the highest to the lowest *R_TI/BI* quintile, which is economically significant when compared to the mean, 3.03 percent, or median, 1.11 percent, for short interest in the sample.³¹ Hence, the effect of *R_TI/BI* on *SI%* is incremental to the effects documented in prior literature (Dechow et al. 2001; Jones and Lamont 2002; D'Avolio 2002; Asquith et al. 2005; Nagel 2005; Hirshleifer et al. 2011).³² The results suggest that short sellers appear to exploit information in the tax fundamental and accumulate larger positions when the firms' *TI/BI* values are lower. The lower the *TI/BI* ratio, the more likely a firm's current earnings will not persist into the future but rather will fall. Hence, as expected, low *R_TI/BI* firms are attractive to short sellers who seek to exploit the slow impounding of *TI/BI* information into share prices.

In Table 6, Panel B we examine whether short interest is more strongly related to *R_TEMP/BI* or *R_PERM/BI*. The coefficients of *R_TEMP/BI* are positive and significant ($p < 0.05$ level or better), while the coefficients of *R_PERM/BI* are insignificant. The coefficient on the interaction of *R_TEMP/BI* and *Post109* is also significantly positive. Overall, the results are consistent with *TEMP/BI* being the main driver of the relation between *SI%* and *TI/BI*.

Evidence that there is short arbitrage of *TI/BI* does not necessarily imply that the arbitrage eliminates the *TI/BI* anomaly. Short arbitrage is costly and risky, as explained in Section II, and therefore short sellers will arbitrage to the point where the expected profits equal the costs. We evaluate whether short arbitrage of *TI/BI* is greater when there is greater ease of arbitrage, such as where the supply of loanable shares is greater, by interacting *TI/BI* with *IO*. Untabulated results indicate that the coefficient on *R_TI/BI* is -0.089 ($t = -3.58$, $p < 0.01$), indicating that when institutional ownership is as low as 1 percent, which is *lnIO* = 0, there is still evidence of short sellers exploiting BTDs. Thus, the effect of *TI/BI* on short interest is significantly greater

³⁰ Table 2, Panel A indicates that mean monthly short interest (*SI%*) is 3.025 (median = 1.107) percent of shares outstanding, similar in magnitude to evidence in prior literature (e.g., Hirshleifer et al. 2011). Untabulated results indicate that mean (median) *SI%* is 0.963 (0.248) percent in the pre-109 period and grows to 3.147 (1.281) percent post-109; correspondingly, there is significant growth in other *SI%* determinants (untabulated), including *IO*, firm size, analyst following, *Leverage*, *STD*, and *Turnover*, and declines in *BM* and *MOM*, as expected.

³¹ The 32.8 percent is calculated as follows: $0.082 * (5 - 1) = 0.328$.

³² We find in untabulated results that including analyst forecast errors in the *SI%* regression does not change our inferences. We also use a change regression specification and our results are robust to potential missing variable bias.

TABLE 6
Multivariate Analysis of Short Interest on Book-Tax Measures

Panel A: *TI/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		4.464	1.56	2.158	0.71
<i>R</i> <i>TI/BI</i>	–	–0.082***	–4.16	–0.096***	–3.31
<i>R</i> <i>TI/BI</i> * <i>Post109</i>				0.039	1.34
<i>Post109</i>				0.739***	5.27
<i>TaxMom</i>		–6.397***	–4.60	–5.465***	–3.51
<i>HighAbnAcc</i> , <i>LowF</i> , and <i>High%OAcc</i>		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year * Firm	
n		78,320		78,320	
Adjusted R ²		12.37%		12.01%	

Panel B: *TEMP/BI* and *PERM/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		1.909	0.67	5.788	0.14
<i>R</i> <i>TEMP/BI</i>	+	0.101***	4.59	0.038**	2.04
<i>R</i> <i>PERM/BI</i>	?	0.018	0.69	0.014	0.60
<i>Post109</i>				0.677***	3.64
<i>R</i> <i>TEMP/BI</i> * <i>Post109</i>	+			0.062***	2.36
<i>R</i> <i>PERM/BI</i> * <i>Post109</i>	?			0.028	1.32
<i>TaxMom</i>		–6.366***	–4.35	–5.051***	–3.37
<i>HighAbnAcc</i> , <i>LowF</i> , and <i>High%OAcc</i>		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year * Firm	
n		78,320		78,320	
Adjusted R ²		12.65%		12.29%	

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels (one-tailed for signed predictions; two-tailed for all else), respectively.

Table 6 presents the results of model (6) on the short arbitrage of *TI/BI* and *TEMP/BI*. The dependent variable is short position five months after the fiscal year-end scaled by the number of shares outstanding in the same month. *R* *TI/BI*, *R* *TEMP/BI*, and *R* *PERM/BI* are the industry-year quintile ranks of *TI/BI*, *TEMP/BI*, and *PERM/BI*, respectively. *HighAbnAcc*, *High%OAcc*, and *LowF* are simultaneously controlled. Other Controls include *lnSize*, *BM*, *Turnover*, *lnIO*, *MOM*, *STD*, *Exchg*, *lnAF*, *Leverage*, and Guenther (2011) variables. The sample consists of NYSE and NASDAQ firms from 1988 to 2009.

Variable definitions are in Table 2.

when shares are more easily borrowed, as proxied by institutional shareholding. Moreover, the coefficient on $R\_TI/BI * lnIO$ is a significant -0.006 ($t = -2.07$, $p < 0.05$), as predicted, implying that as the constraint on loanable shares is relaxed, there is an increase in short selling targeting low TI/BI . In addition, the interactive variable $R\_TEMP/BI * lnIO$ also indicates that there is significantly greater short selling of temporary BTDs when there is higher availability of loanable shares.

Return Asymmetry for TI/BI and $TEMP/BI$ Anomalies

As discussed earlier, we examine the effectiveness of short arbitrage in correcting mispricing by estimating the return asymmetry measure, $-(R_H + R_L)$, between the short and long sides of the TI/BI and $TEMP/BI$ anomalies. A larger absolute value of the excess returns of the short portfolio, compared to that of the long portfolio, will increase the magnitude of $-(R_H + R_L)$. For TI/BI , Table 4, Panel A indicates the return asymmetry in the full sample is an economically significant 42 basis points per month ($t = 4.05$, $p < 0.01$). That is, the negative mean abnormal return of -57 basis points per month in the lowest TI/BI quintile is almost four times as large in absolute value as the positive mean return of 15 basis points per month among firms in the highest TI/BI quintile. For comparison, the accruals anomaly in our sample presents a returns asymmetry of 49 basis points per month, similar to Hirshleifer et al. (2011).

The return asymmetry for the joint $AbnAcc$ and TI/BI trading strategy in Table 4, Panel B is significant and even larger, 71 basis points per month ($t = 3.58$, $p < 0.01$). The negative mean abnormal return of -115 basis points per month for firms in quintile 1 of TI/BI and quintile 5 of $AbnAcc$ is 2.6 times larger in absolute value than the mean return of 44 basis points per month for the portfolio of firms in the top quintile of TI/BI and bottom quintile of $AbnAcc$.

Regarding $TEMP/BI$, Table 4, Panel A indicates that the return asymmetry is a substantial 17 basis points per month, which is significant at the $p < 0.01$ level ($t = 2.74$). The negative mean abnormal return of -32 basis points per month in the high $TEMP/BI$ quintile is more than twice as large in absolute value as the mean return of 15 basis points per month among firms in the lowest $TEMP/BI$ quintile. The return asymmetry for the joint $AbnAcc$ and $TEMP/BI$ strategy in Table 4, Panel B is 50 basis points per month, which is significant at the $p < 0.01$ level ($t = 2.98$). In sum, the evidence suggests that while TI/BI and $TEMP/BI$ mispricing effects are arbitrated, substantial mispricing of these variables remains, likely due to high costs of arbitrage.³³

VI. INSIDER TRADING ANALYSES

Empirical Models

Based on prior research, we estimate regression (7) to test the relation between insider trading and TI/BI as to whether insiders exploit the BTD anomalies:

$$IT\%_t = a_0 + a_1R\_TI/BI_{t-1} + a_2X_{t-1} + a_3TaxMom_{t-1} + a_4lnSize_{t-1} + a_5RDCAPEX_{t-1} + a_6G_{t-1} + a_7Litigation_t + a_8lnAF_{t-1} + a_9lnIO_{t-1} + a_{10}STD_{t-1} + a_{11}MOM_t + a_{12}BM_{t-1} + a_{13}Guenther_{t-1} + \varepsilon. \tag{7}$$

If insiders use R_TI/BI as an indicator of future earnings, then the net sales of their firms' shares should decrease with R_TI/BI and increase with R_TEMP/BI . We use $IT\%$ as defined in Section III and expect that $a_1 < 0$ in regression (7). To test for insider trading on the BTD components, we

³³ Return asymmetry analyses using joint sorts with $\%OAcc$, $F-Score$, and $TaxMom$ do not change our inferences.

replace focus on R_TEMP/BI and R_PERM/BI and expect a positive coefficient on R_TEMP/BI . In addition to other accounting-based predictor variables, X and $TaxMom$, and the *Guenther* variables, we control for other factors affecting insider trading (Thevenot 2011; Huddart and Ke 2007; Richardson et al. 2004; Roulstone 2003). *Litigation* is an indicator variable that equals 1 if a firm is in industries with high litigation risks,³⁴ and $RDCAPEX$, our investment variable, and G , earnings growth, are as previously defined. We also include controls from the short interest model, size, BM , IO , STD , and MOM . We estimate model (7) as a pooled OLS regression with year fixed effects, and standard errors are clustered by firm to address potential serial correlation problems (Petersen 2009; Gow et al. 2010).

Insider Trading Results

Table 7 displays the insider trading results. As predicted, the coefficients on R_TI/BI in Panel A are negative and significant at the $p < 0.01$ level for $IT\%$ in both regressions. The magnitude is also economically significant. Net insider sales scaled by total shares outstanding of low R_TI/BI firms are 1.476 times higher, controlling for other accounting predictors than that for high R_TI/BI firms.³⁵ The results suggest that insiders also exploit information in the tax fundamental and sell larger positions when their firms' TI/BI values are lower. Hence, as expected, low R_TI/BI firms are attractive to insider net selling and, thus, it appears that insiders act as if they exploit the slow impounding of TI/BI information into share prices.

Decomposing TI/BI into its temporary and permanent BTD components reveals, in Table 7, Panel B, that the coefficients on R_TEMP/BI are positive and significant at the $p < 0.05$ level or better, but not the permanent component. Consistent with the short interest results, the positive relation between $IT\%$ and $TEMP/BI$ is significantly stronger under SFAS No. 109.³⁶

VII. ADDITIONAL SENSITIVITY ANALYSIS

In this section, we summarize untabulated results for additional sensitivity checks of our main results. We initially relax the assumption of using the highest U.S. corporate income tax rate for all firms. First, we drop observations having a ratio of foreign income-to-domestic plus foreign income greater than 50 percent, since firms with extensive foreign operations could imply differing tax rates, and also drop observations having a ratio of R&D expenditures-to-total sales in the upper quartile, which implies a high likelihood of R&D tax credits (Ayres, Jiang, and Laplante 2009). All results are qualitatively similar, except $PERM/BI$ is significant ($p < 0.1$). Second, we compute TI and BI on a pre-tax basis (Hanlon et al. 2012), and our inferences are unchanged. We also exclude observations from 2007–2009 when FASB Interpretation No. 48 (FIN 48), *Accounting for Uncertainty in Income Taxes* (FASB 2006), was in effect during our sample period. In cases where it is unlikely aggressive tax positions will be upheld, the firm's reported current tax expense will not be reduced by the uncertain tax benefits. If so, our estimate of TI from the reported current tax expense will overstate TI .³⁷ Our inferences are unaffected.

³⁴ High litigation risk four-digit SIC industries are 2833, 2836, 3570, 3577, 3600–3674, 5200–5961, and 7370–7374.

³⁵ The spread of 1.476 is between the lowest quintile TI/BI , $-0.369 * 1$, and the highest quintile of TI/BI , $-0.369 * 5$ from Table 7, Panel A of insider net sales of their firms' own stock as a fraction of total shares outstanding, equivalent to one standard deviation (1.3458) of scaled insider net sales.

³⁶ Using a 0/1 indicator that equals 1 if insiders are net sellers within 20 days after earnings announcements, instead of $IT\%$, yields the same inferences in the TI/BI and component analyses (untabulated). The results are also robust using change in $IT\%$.

³⁷ We thank Bob Lipe for providing us with a detailed example illustrating possible increased measurement error.

TABLE 7

Multivariate Analysis of Insider Trading on Book-Tax Measures

Panel A: *TI/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		1.402***	8.34	1.419***	8.55
<i>R_TI/BI</i>	—	−0.369***	−2.92	−0.273***	−4.09
<i>R_TI/BI * Post109</i>				−0.076	−0.75
<i>Post109</i>				0.027***	4.02
<i>TaxMom</i>		−2.672***	−3.01	−2.985***	−3.24
<i>R_AbnAcc</i> , <i>R_%OAcc</i> , and <i>F-Score</i>		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year * Firm	
n		47,291		47,291	
Adjusted R ²		4.27%		3.84%	

Panel B: *TEMP/BI* and *PERM/BI*

Variables	Prediction	Overall Effect		Incremental SFAS No. 109 Effect	
		Coeff.	t-stat	Coeff.	t-stat
Intercept		1.496***	11.5	1.397***	11.59
<i>R_TEMP/BI</i>	+	0.269**	2.04	0.536**	2.31
<i>R_PERM/BI</i>	?	0.004	0.13	0.006	0.25
<i>Post109</i>				0.053**	2.30
<i>R_TEMP/BI * Post109</i>	+			0.103***	8.04
<i>R_PERM/BI * Post109</i>	?			0.003	0.63
<i>TaxMom</i>		−2.971***	−3.33	−2.976***	−3.33
<i>R_AbnAcc</i> , <i>R_%OAcc</i> , and <i>F-Score</i>		Yes		Yes	
Other Controls		Yes		Yes	
Regression		OLS		OLS	
Year Fixed Effects		Yes		No	
Standard Errors		Clustered by Firm		Clustered by Year*Firm	
n		47,291		47,291	
Adjusted R ²		4.52%		4.36%	

*, **, *** Denote significance at the 0.10, 0.05, and 0.01 levels (one-tailed for signed predictions; two-tailed for all else), respectively.

Table 7 presents the results of model (7), insider trading on *TI/BI* and *TEMP/BI*. The dependent variable is the net number of shares sold by insiders within 20 days after earnings announcements scaled by the number of shares outstanding at the fiscal year-end. *R_TI/BI*, *R_TEMP/BI*, and *R_PERM/BI* are the industry-year quintile ranks of *TI/BI*, *TEMP/BI*, and *PERM/BI*, respectively. *R_AbnAcc*, *R_%OAcc*, and *F-Score* are simultaneously controlled. Other Controls include *lnSize*, *BM*, *RDCAPEX*, *G*, *Litigation*, *lnIO*, *MOM*, *STD*, *lnAF*, and Guenther (2011) variables. The sample consists of NYSE and NASDAQ firms from 1988 to 2009.

Variable definitions are in Table 2.

Rather than using the ratio forms for BTD variables, we compute the difference, $BI-TI$, and scale total, temporary, and permanent BTDs by lagged total assets as alternative measures of TI/BI , $TEMP/BI$, and $PERM/BI$. The results largely support our inferences.³⁸

We investigate the robustness of our results to controlling for asset-scaled total accruals or cash flows from operations instead of abnormal accruals. Replacing R_AbnAcc with the ranks of total accruals does not qualitatively impact our results except the coefficient on R_PERM/BI in the short interest regression becomes marginally significant when we substitute high ranked total accruals for $HighAbnAcc$. Controlling for cash flows from operations also does not affect any of our inferences regarding TI/BI or $TEMP/BI$, but we obtain a significant result for the coefficients on $PERM/BI$ in the earnings growth, short interest, and insider trading regressions.

In the short interest regression, we replace the accounting-based predictors with the 11 short interest determinants used by Drake et al. (2011). Our results for R_TI/BI , R_TEMP/BI , and R_PERM/BI are unaffected.³⁹ Finally, we consider institutional investors as another group of sophisticated investors. We replace $IT\%$ with the change in institutional holdings from the end of the quarter preceding an annual earnings announcement to the end of the quarter following the annual earnings announcement. The results are qualitatively identical to those in Table 7.

VIII. SUMMARY AND CONCLUSIONS

In this study, we provide a comprehensive analysis of the predictability of future earnings growth and future abnormal returns by the BTD ratio TI/BI (as in Lev and Nissim 2004) and its temporary and permanent components. Hanlon and Heitzman (2010) and Graham et al. (2012) view the results in Lev and Nissim (2004) as mixed or weak. We improve on past studies in various ways. We use a larger sample that covers a longer time period post-109 so that our sample period is not dominated by the dot-com bubble years. We also include firms with non-December year-ends. Our improved test designs feature rolling monthly cross-sectional Fama-MacBeth panel regressions, joint sorts to estimate conservative *incremental* hedge profits from other accounting predictors, and extensive risk controls. By employing an extensive set of known accruals-related predictors, including abnormal accruals, percent operating accruals, *F-Score*, tax momentum, and *Guenther* variables as controls we demonstrate *incremental* predictability of BTDs. Our results provide strong evidence of a TI/BI pricing anomaly and that the temporary BTD component, $TEMP/BI$, is the primary source; the latter is a new result in the literature.

In addition, we provide new evidence from short selling and insider trading that suggests that sophisticated investors and managers exploit their superior knowledge of BTD information about future cash flows. To our knowledge, this is the first study to relate BTDs to short selling and insider trading. The evidence of short arbitrage presents additional direct evidence for BTD mispricing. Extending Weber's (2009) finding for TI/BI , we find that the $TEMP/BI$ anomaly is also stronger when fewer financial analysts follow a firm.

Investor mispricing of BTDs can be explained by the limited attention theory for accounting anomalies (Hirshleifer and Teoh 2003). Investors with limited attention may ignore value-relevant information contained in BTDs, are unlikely to discount appropriately for agency incentives, and so are likely to mis-value the firm. Investor limited attention can derive either from limited information-processing resources or from limited cognitive power. Taxable income item is not directly reported in the financial statements and requires investor time and effort to obtain and process. Inattentive investors therefore may miss the incremental insight about future fundamentals

³⁸ We also calculate these asset-scaled BTB measures on a pre-tax basis instead of an after-tax basis.

³⁹ We also augment the insider trading regression with the Drake et al. (2011) variables and, again, our key variable results are unchanged.

that can be inferred by considering both taxable income and book income together, beyond what can be learned by observing only book income. In addition to firm future fundamentals, temporary BTDs particularly can contain information about agency issues related to earnings management and/or tax management that are missed by inattentive investors. In sum, valuing BTDs, either overall or separately for the temporary and permanent components, demands considerable cognition and processing resources.

We provide strong evidence that the capital market mis-values *TI/BI* and *TEMP/BI*. Disclosure of a reconciliation of taxable income to book income, similar to one that is included in the corporate income tax return (Mills and Plesko 2003), may increase salience of BTD information and reduce uncertainty about the estimates of total BTDs and its major components. Since investors with limited attention are less likely to overlook salient information, they would be less likely to ignore BTD information about future fundamentals. A big benefit of such a “sunshine policy” would likely be reduced BTD mispricing and improved capital market resource allocation. Of course, we recommend further study of the costs and unintended consequences of such a sunshine policy before recommending it to policy makers for adoption.

REFERENCES

- Accounting Principles Board (APB). 1967. *Accounting for Income Taxes*. Opinions of the Accounting Principles Board No. 11. American Institute of Certified Public Accountants. New York, NY: AICPA.
- Ali, A., and M. Trombley. 2006. Short sales constraints and momentum in stock returns. *Journal of Business, Finance, and Accounting* 33: 587–615.
- Asquith, P., P. Pathak, and J. Ritter. 2005. Short interest, institutional ownership, and stock returns. *Journal of Financial Economics* 78: 243–276.
- Ayres, B., X. Jiang, and S. Laplante. 2009. Taxable income as a performance measure: The effects of tax planning and earnings quality. *Contemporary Accounting Research* 26 (1): 15–54.
- Badertscher, B., J. Phillips, M. Pincus, and S. Rego. 2009. Earnings management strategies and the trade-off between tax benefits and detection risk: To conform or not to conform? *The Accounting Review* 84 (1): 63–97.
- Blaylock, B., T. Shevlin, and R. Wilson. 2012. Tax avoidance, large positive book-tax differences, and earnings persistence. *The Accounting Review* 87 (1): 91–120.
- Chanos, J. 2008. Short sellers keep the market honest. *Wall Street Journal* (September 22).
- Cohen, L., K. Diether, and C. Malloy. 2007. Supply and demand shifts in the shorting market. *Journal of Finance* 62 (5): 2061–2096.
- Daniel, K., M. Grinblatt, S. Titman, and R. Wermers. 1997. Measuring mutual fund performance with characteristic-based benchmarks. *Journal of Finance* 52 (3): 1035–1058.
- D’Avolio, G. 2002. The market for borrowing stock. *Journal of Financial Economics* 66: 271–306.
- Dechow, P., R. Sloan, and A. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.
- Dechow, P., R., W. Ge, and C. Schrand. 2010. Understanding earnings quality: A review of the proxies, their determinants and their consequences. *Journal of Accounting and Economics* 50:344–401.
- Dechow, P., R., A. Hutton, L. Meulbroek, and R. Sloan. 2001. Short-sellers, fundamental analysis and stock returns. *Journal of Financial Economics* 61: 77–106.
- Desai, H., S. Krishnamurthy, and K. Venkataraman. 2006. Do short sellers target firms with poor earnings quality? Evidence from earnings restatements. *Review of Accounting Studies* 11:71–90.
- Dhaliwal, D., H. Lee, M. Pincus, and L. Steele. 2012. *Taxable Income Properties, Predictability of Book Income, and Firm Risk*. Working paper, The University of Arizona, Lehigh University, University of California, Irvine, and University of Connecticut.

- Diamond, D., and R. Verrecchia. 1987. Constraints on short-selling and asset price adjustment to private information. *Journal of Financial Economics* 18: 277–311.
- Dolgoplov, S. 2008. Insider trading. *The Concise Encyclopedia of Economics*. Library of Economics and Liberty. Available at: <http://www.econlib.org/library/Enc/InsiderTrading.html>
- Drake, M., L. Rees, and E. Swanson. 2011. Should investors follow prophets or bears? Evidence on the use of public information by analysts and short sellers. *The Accounting Review* 86: 101–130.
- Fama, E., and J. MacBeth. 1973. Risk, return and equilibrium: Empirical tests. *Journal of Political Economy* 81: 607–636.
- Fidler, S. 2010. EU draft rules target short selling. *Wall Street Journal* (September 15).
- Financial Accounting Standards Board (FASB). 1987. *Accounting for Income Taxes*. Statement of Financial Accounting Standards No. 96. Financial Accounting Standards Board. Norwalk, CT: FASB.
- Financial Accounting Standards Board (FASB). 1992. *Accounting for Income Taxes*. Statement of Financial Accounting Standards No. 109. Financial Accounting Standards Board. Norwalk, CT: FASB.
- Financial Accounting Standards Board (FASB). 2006. *Accounting for Uncertainty in Income Taxes, and Interpretation of FASB Statement No. 109*. FASB Interpretation No. 48. Financial Accounting Series. Norwalk, CT: FASB.
- Gleason, C., and L. Mills. 2002. Materiality and contingent tax liability reporting. *The Accounting Review* 11 (2): 317–342.
- Gow, I., G. Ormazabal, and D. Taylor. 2010. Correcting for cross-sectional and time-series dependence in accounting research. *The Accounting Review* 85 (2): 483–512.
- Graham, J., J. Raedy, and D. Shackelford. 2012. Research in accounting for income taxes. *Journal of Accounting and Economics* 53: 412–434.
- Guenther, D. 2011. *What Do We Learn From Large Book-Tax Differences?* Working paper, University of Oregon.
- Hanlon, M. 2005. The persistence and pricing of earnings, accruals, and cash flows when firms have large book-tax differences. *The Accounting Review* 80 (1): 137–166.
- Hanlon, M., and S. Heitzman. 2010. A review of tax research. *Journal of Accounting and Economics* 50:127–178.
- Hanlon, M., G. Krishnan, and L. Mills. 2012. Audit fees and book-tax differences. *The Journal of the American Taxation Association* 34:55–86.
- Hanlon, M., S. Laplante, and T. Shevlin. 2005. Evidence for the possible information loss of conforming book income and taxable income. *Journal of Law and Economics* 48 (2): 407–442.
- Hatzalla, N., R. Lundholm, and E. Van Winkle. 2011. Percent accruals. *The Accounting Review* 86: 209–236.
- Hayn, C. 1995. The information content of losses. *Journal of Accounting & Economics* 20: 125–153.
- Hirshleifer, D., and S. Teoh. 2003. Limited attention, information disclosure, and financial reporting. *Journal of Accounting & Economics* 36: 337–386.
- Hirshleifer, D., S. H. Teoh, and J. Yu. 2011. Short arbitrage, return asymmetry and the accrual anomaly. *Review of Financial Studies* 24 (7): 2429–2461.
- Hribar, P., and D. Collins. 2002. Errors in estimating accruals: Implications for empirical research. *Journal of Accounting Research* 40 (1): 105–134.
- Huddart, S., and B. Ke. 2007. Information asymmetry and cross-sectional determinants of insider trading. *Contemporary Accounting Research* 24 (1): 195–232.
- Jones, C., and O. Lamont. 2002. Short sale constraints and stock returns. *Journal of Financial Economics* 66: 207–239.
- Jones, J. 1991. Earnings management during import relief investigation. *Journal of Accounting Research* 29 (2): 193–228.
- Karpoff, J., and X. Lou. 2010. Short sellers and financial misconduct. *Journal of Finance* 40 (1): 1879–1913.
- Ke, B., S. Huddart, and K. Petroni. 2003. What insiders know about future earnings and how they use it: Evidence from insider trades. *Journal of Accounting and Economics* 35 (3): 315–346.
- Lauricella, T. 2009. Lehman legacy alters global markets. *Wall Street Journal* (September 14).

- Lev, B., and D. Nissim. 2004. Taxable income, future earnings, and equity values. *The Accounting Review* 79 (4): 1039–1074.
- Mills, L., and G. Plesko. 2003. Bridging the reporting gap: A proposal for more informative reconciling of book and tax income. *National Tax Journal* 56 (4): 865–894.
- Nagel, S. 2005. Short sales, institutional investors and the cross-section of stock returns. *Journal of Financial Economics* 78: 277–309.
- Petersen, M. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies* 22 (1): 435–480.
- Phillips, J., M. Pincus, and S. Rego. 2003. Earnings management: New evidence based on deferred tax expense. *The Accounting Review* 78 (2): 491–521.
- Piotroski, J. 2000. Value investing: The use of historical financial statement information to separate winners from losers. *Journal of Accounting Research* 38 (Supplement): 1–41.
- Piotroski, J., and D. Roulstone. 2005. Do insider trades reflect both contrarian beliefs and superior knowledge about future cash-flow realizations? *Journal of Accounting & Economics* 39 (1): 55–82.
- Piotroski, J., and E. So. 2012. Identifying expectation errors in value/glamour strategies: A fundamental analysis approach. *Review of Financial Studies* 25 (9): 2841–2875.
- Pulliam, S., and C. Bray. 2011. Trader draws record sentence. *Wall Street Journal* (October 13).
- Pulliam, S., M. Rothfeld, J. Strasbur, and G. Zuckerman. 2010. U.S. in vast insider trading probe. *Wall Street Journal* (November 20).
- Richardson, S., S. H. Teoh, and P. Wysocki. 2004. The walkdown to beatable analyst forecasts: The roles of equity issuance and insider trading incentives. *Contemporary Accounting Research* 21 (4): 885–924.
- Roulstone, D. 2003. The relation between insider-trading restrictions and executive compensation. *Journal of Accounting Research* 41 (3): 525–553.
- Securities and Exchange Commission (SEC). 2010. SEC Approves short selling restrictions. *Press Release* (February 24). Available at: <http://www.sec.gov/news/press/2010/2010-26.htm>
- Stambaugh, R., J. Yu, and Y. Yuan. 2012. The short of it: Investor sentiment and anomalies. *Journal of Financial Economics* 104: 288–302.
- Teoh, S. H., I. Welch, and T. Wong. 1998a. Earnings management and the long-run market performance of the initial public offering. *Journal of Finance* 53 (6): 1935–1974.
- Teoh, S. H., I. Welch, and T. Wong. 1998b. Earnings management and the underperformance of seasoned equity offerings. *Journal of Financial Economics* 50: 63–99.
- Thevenot, M. 2011. The factors affecting illegal insider trading in firms with violations of GAAP. *Journal of Accounting & Economics* 53: 375–390.
- Thomas, A. 2010. Germany passes diluted “naked” ban. *Wall Street Journal* (July 2).
- Thomas, J., and F. Zhang. 2011. Tax expense momentum. *Journal of Accounting Research* 49 (3): 791–821.
- U.S. Attorney’s Office, Eastern District of New York (USAO EDNY). 2006. Former FBI agent sentenced to six years of imprisonment for racketeering and securities fraud. *Press Release* (October 2). Available at: <http://www.justice.gov/usao/nye/pr/2006/2006Oct02.html>
- Weber, D. 2009. Do analysts and investors fully appreciate the implications of book-tax differences for future earnings? *Contemporary Accounting Research* 26 (4): 1175–1206.
- Xie, H. 2001. The mispricing of abnormal accruals. *The Accounting Review* 76 (31): 357–373.

Can Offering a Signing Bonus Motivate Effort? Experimental Evidence of the Moderating Effects of Labor Market Competition

Jongwoon (Willie) Choi
University of Pittsburgh

ABSTRACT: Employers often rely on informal controls such as trust to motivate organizationally desirable behaviors from their workers by appealing to the latter's reciprocity. Notably, trust and reciprocity can promote a "gift exchange" between employers and workers. Using an experiment, I investigate whether labor market competition moderates the emergence of a gift exchange in labor markets in which signing bonus offers serve as a potential signal of trust and the duration of the employment relationship is endogenously determined. I find that offering a signing bonus more positively affects both workers' beliefs about the employer's trust in them and their effort when there is an excess supply of workers than when there is an excess demand for workers. I also find that the initial effects of signing bonuses may not persist over time. Additional analyses suggest that both employers' and workers' expectations may affect whether and how trust and reciprocity develop over time.

Keywords: *trust; reciprocity; signing bonus; labor market competition; attribution theory.*

This paper is based on my dissertation completed at Emory University. I thank my dissertation chair, Kristy Towry, and my other committee members, Gary Hecht, Kathryn Kadous, and Greg Waymire, for their advice, guidance, and support. I also thank Lisa Koonce (editor), John Harry Evans III (senior editor), two anonymous reviewers, Jason Brown, Eric Chan, Peter Demerjian, Annie Farrell, Ryan Hamilton, Thomas Hemmer, Vicky Hoffman, Jason Kuang, Joan Luft, Melanie Millar, Don Moser, Drew Newman, Grace Pownall, Shiva Rajgopal, Marcel van Rinsum, Ted Rodgers, Mike Shields, Ivo Taffkov, Bill Tayler, Jane Thayer, Xue Wang, Michael Williamson, Donnie Young, May Zhang, brownbag participants at Emory University and Georgia State University, workshop participants at Emory University and the University of Pittsburgh, participants at the 2012 AAA Annual Meeting, the 2012 MAS Research and Case Conference, and the 2012 ABO Research Conference, and students in the Experimental Research in Accounting doctoral seminar at the University of Pittsburgh for their comments and suggestions. I gratefully acknowledge excellent programming assistance from Eric Best, and financial support from Goizueta Business School, the James T. Laney School of Graduate Studies, the Sheth Fellowship, the Goizueta Fellowship, the University of Pittsburgh, and the Korean American Scholarship Foundation.

Editor's note: Accepted by Lisa Koonce.

Submitted: November 2011
Accepted: October 2013
Published Online: October 2013

I. INTRODUCTION

To motivate organizationally desirable behaviors from their workers, employers rely not only on formal controls such as financial incentives and audits, but also on informal controls such as trust. Research demonstrates that trust plays a key role in shaping the outcomes of employment relationships, with reciprocity underlying this effect (Arrow 1970; Berg, Dickhaut, and McCabe 1995; Fehr 2009). For example, evidence suggests that trust and reciprocity can facilitate a “gift exchange” between employers and workers (Akerlof 1982; Fehr, Kirchsteiger, and Riedl 1993) in which the two parties extract greater mutual gains from the employment relationship compared to when trust and reciprocity are absent. Given the potential effects, knowledge of factors that affect the emergence of a gift exchange is vital when seeking to harness the benefits of trust.

Employers can convey their trust in their workers and initiate a gift exchange with their contract choices (Akerlof 1982). For example, prior research on labor markets finds that when employers give workers a gift of wages above the market-clearing level, workers reciprocate with a gift of effort above the enforceable level (Fehr, Goette, and Zehnder 2009). Notably, employers may not be limited to offering a gift of above-market wages. Specifically, a signing bonus can also be viewed as a gift (Allen 1997; Van Wesep 2010). However, the use of signing bonuses to signal trust and initiate a gift exchange has not been explored.

Several features of signing bonuses facilitate their use as a signal of trust. Employers convey their trust in their workers by taking actions that put them at risk of being harmed by their workers. Since signing bonuses are paid early in a worker’s tenure and can be viewed as an accelerated payment of compensation that would otherwise be paid in the future (Poe 1999; Arya, Frimor, and Mittendorf 2003; Van Wesep 2010), a contract that includes a signing bonus pays the worker more compensation at the outset of the employment relationship than a contract without the bonus. Thus, employers incur greater labor costs when they offer the bonus than when they do not if, for example, the agency relationship ends sooner than the employer expected.

In order for the offer of a signing bonus to positively affect worker effort, workers must believe that the employer trusts them to a greater extent when the bonus is offered than when it is not. However, rather than being constant, the extent to which a signing bonus conveys trust may be affected by the broader market environment in which the bonus is offered (Bowles 1998). In this study, I use an experiment to investigate whether the effects of offering a signing bonus on worker effort depend on labor market competition, i.e., whether there is an excess demand for labor or an excess supply of labor. Competition is a prominent feature of many market settings, and is closely linked with signing bonuses (Poe 1999). Specifically, labor market competition often induces market participants to engage in a bidding war in response to competitive pressures (Roth, Prasnikar, Okuno-Fujiwara, and Zamir 1991), and employers often respond to such pressures by offering signing bonuses (Goolsby and Unmuth 2008; Miller 2008). Thus, workers are more likely to be offered a signing bonus when there is excess demand than when there is excess supply.

Importantly, the greater competitive pressure that employers face when there is an excess demand for labor offers an alternative explanation for a signing bonus offer. In particular, while workers may attribute a signing bonus offer to the employer’s trust in them in both labor market competition conditions, when there is an excess demand for labor, workers may also attribute the signing bonus offer to the employer’s desire to avoid getting shut out of the market (Bauernschuster, Falck, and Große 2013). This attribution does not apply when there is an excess supply of labor. Due to the greater number of plausible attributions that can be made when there is excess demand, workers are less likely to attribute the signing bonus offer to the employer’s trust in them when there is excess demand than when there is excess supply (Kelley 1973). Since individuals often rely on their attributions to determine their own behaviors (Rabin 1993; Falk, Fehr, and Fischbacher 2008), I hypothesize that a signing bonus offer positively affects both

workers' beliefs regarding the employer's trust in them and their effort to a greater extent when there is an excess supply of labor than when there is an excess demand for labor.

I examine the interactive effects of signing bonus offers and labor market competition using experimental labor markets in which participants assume the role of either employer or worker, and interact with each other anonymously. In each market, employers hire a single worker to provide effort for up to two periods. After employers observe their worker exert effort in the first period, they decide whether to retain the worker. Retained workers then exert effort in the second period. I manipulate labor market competition by varying whether labor markets include two employers and one worker (excess demand) or one employer and two workers (excess supply). I also manipulate a second factor, contract choice constraint, at two levels. When contract choices are not constrained, employers who offer a contract have the option to include a signing bonus in the contract. Employers offering a signing bonus pay more in the first period than those not offering the bonus. In contrast, when contract choices are constrained, signing bonuses cannot be offered. The contract choice constraint manipulation establishes benchmarks against which the effects of offering a signing bonus can be compared. In addition, letting employers decide whether to retain the worker creates an environment in which the duration of the employment relationship is endogenously determined, which allows me to explore whether the effects of offering a signing bonus persist over time.

Consistent with the hypotheses, I find that when there is an excess supply of labor, offering a signing bonus more positively affects workers' beliefs about the employer's trust in them as well as their effort in the first period. I also test the hypotheses using path analysis to estimate the links among signing bonus offers, workers' trust beliefs, and effort across labor market competition conditions. The path analysis results provide additional support for the hypotheses. That is, offering a signing bonus more positively affects worker effort when there is an excess supply of labor than when there is an excess demand for labor. Further, the impact of signing bonus offers on workers' beliefs regarding the employer's trust in them drives this effect, in that workers attribute a signing bonus offer to the employer's trust in them to a greater extent when there is an excess supply of labor than when there is an excess demand for labor.

To examine whether the effects of offering a signing bonus persist over time, I examine whether employers' retention decisions differ across conditions, and whether being retained affects workers' subsequent trust beliefs and effort. I find that that these variables do not differ across conditions, suggesting that the initial effects of offering a signing bonus may not persist over time. Additional analyses suggest that employers' and workers' expectations may play an important role in how trust and reciprocity develop over time. Specifically, when there is an excess supply of labor, employers who offer a signing bonus expect greater effort from their workers than they do when either no signing bonus is offered or when there is an excess demand for labor. Also, while a signing bonus more positively affects effort when there is excess labor supply, workers receiving a signing bonus when there is excess supply are less likely to fulfill their employer's greater effort expectations. Fulfilling employer's effort expectations appears to be an important factor for retention, as a majority of workers who fulfill their employer's expectations were retained. Finally, workers receiving a signing bonus seem to assess greater prospects of being retained when there is excess supply than when either there is excess demand or when a signing bonus is not offered. These differential expectations affect how trusted workers feel after being retained and influence their subsequent effort response.

The insights from my study facilitate greater understanding of the use of trust as an informal control mechanism and the emergence of a gift exchange between employers and workers. While the benefits of trust and reciprocity are well known, knowledge of when these benefits are likely to materialize is limited. My results suggest that labor market competition is one factor that affects whether employers and workers consummate a gift exchange. In doing so, my findings underscore

the moderating effects of the contextual environment on workers' responses to their employer's trusting actions (Sen 1997; Bowles 1998; Falk, Fehr, and Zehnder 2006; Kuang and Moser 2009). Furthermore, the results suggest that expectations may play a critical role in whether and how trust and reciprocity develop over time. Finally, I contribute to the literature on the effort-inducing effects of signing bonuses. Prior research emphasizes the worker's marginal productivity of effort as a mechanism by which signing bonus offers positively affect effort. For example, Arya et al. (2003) suggest that offering a signing bonus can motivate workers to invest more in their human capital, which increases the marginal productivity of their effort. In addition, Van Wesep (2010) suggests that offering a signing bonus can signal an employer's belief in a good match with a worker, where a good match implies a greater marginal productivity of effort than a bad match. My study complements this research by showing that a signing bonus offer can positively affect effort independently of the worker's marginal productivity of effort by conveying trust and appealing to the worker's reciprocity.

I develop the theory and hypotheses in Section II, and outline the experiment in Section III. Results are reported in Section IV, and Section V concludes.

II. THEORY AND HYPOTHESES

Trust

This study investigates the moderating effects of labor market competition on the links among signing bonus offers, trust, and worker effort. Consistent with prior research (Mayer, Davis, and Schoorman 1995; Rousseau, Sitkin, Burt, and Camerer 1998; Sobel 2002), I define trust as the willingness to be vulnerable to the actions of another party based on positive expectations of that party's intentions and/or behavior. This definition highlights two features central to the notion of trust. First, trust pertains to relational risk, i.e., susceptibility to being harmed by others (Berg et al. 1995; Rousseau et al. 1998; Das and Teng 2001; Cox 2004). Second, the willingness to be exposed to relational risk stems from the trusting party's positive expectations of the trusted party. These expectations broadly reflect a belief that the trusted party, when faced with the opportunity to either benefit or harm the trusting party, will choose the beneficial actions. In employment relationships, these positive expectations can include an employer's belief that the worker will not shirk even when shirking cannot be detected.

Trust can have a major impact on the realized outcomes of economic transactions (Arrow 1970; Fehr 2009), with reciprocity underlying these effects. When trusted by another party, the trusted party often reciprocates that trust with trustworthy behavior, even at a personal cost (Das and Teng 1998; Sobel 2005; Dohmen, Falk, Huffman, and Sunde 2009). Notably, trust and reciprocity can promote a "gift exchange" between employers and workers (Akerlof 1982; Fehr et al. 1993) that enhances the two parties' mutual gains from the employment relationship.

Gift exchange between transacting partners has been observed in both the investment game (Berg et al. 1995) and experimental labor markets (see Fehr et al. 2009). In these settings, trust leads to actions that put the trusting party at risk of being harmed by the trusted party, while reciprocity leads to trustworthy actions that benefit the trusting party. In the investment game setting, first movers often convey trust by transferring a positive amount to the second mover, who reciprocates the first mover's trust by returning an amount greater than that originally sent by the first mover, increasing the first mover's payoff (Berg et al. 1995; McCabe, Rigdon, and Smith 2003; Cox 2004; Bacharach, Guerra, and Zizzo 2007). Likewise, studies examining behavior in experimental labor markets report that employers convey trust by offering wages above the minimum level, with workers reciprocating with effort levels that generate greater returns for the employer than if the employer had offered the lowest possible wage (Fehr et al. 1993; Fehr, Kirchsteiger, and Riedl 1998; Hannan 2005; Kuang and Moser

2009). In demonstrating the benefits of trust and reciprocity, these studies also highlight the need to understand when such effects are likely to materialize. I examine this issue within a setting in which signing bonuses serve as a potential signal of trust.

Signing Bonuses

Signing bonuses are a popular component of compensation contracts (Uchitelle 1998; Poe 1999; WorldatWork 2009). Although signing bonuses are perhaps better known for being offered to professional athletes and senior executives, their use has become commonplace in a wide range of industries at many levels of the organization. Signing bonuses are offered in industries such as public accounting, engineering, utilities, and education (Uchitelle 1998; Hansen 2005; National Association of Colleges and Employers 2008). Moreover, employers offer signing bonuses not only to workers applying for upper management positions, but also to those applying for middle management, technical staff, and clerical staff positions (WorldatWork 2005, 2009).

Signing bonuses are predominantly cash payments to workers for agreeing to join the firm. They are generally paid early in the worker's tenure with the firm, e.g., in the worker's first paycheck (Allen 1997; WorldatWork 2009; Van Wesep 2010). Furthermore, signing bonuses can be viewed as an accelerated payment of compensation that would otherwise be paid in the future (Allen 1997; Arya et al. 2003; WorldatWork 2007; Van Wesep 2010). In particular, practitioner literature and popular press articles indicate that rather than offer, for example, a higher salary, employers will sometimes choose to offer a signing bonus instead (Uchitelle 1998; Poe 1999).¹ As a result, workers earn more compensation at the outset of an employment relationship that includes a signing bonus.

A popular explanation for the use of signing bonuses is that they improve worker recruitment (Uchitelle 1998; Poe 1999; WorldatWork 2007).² In a recent survey, 88 percent of employers who offer signing bonuses report that such bonuses help them attract workers to their firm (WorldatWork 2000). However, a signing bonus offer may also signal an employer's trust and positively affect worker effort.³ Offering a signing bonus conveys trust because doing so puts employers at greater risk of being harmed by their workers. To illustrate, suppose that an employment relationship ends earlier than the employer expected when the worker was hired. This could occur either because the worker quit soon after being hired, or because the worker's performance was below expectations, prompting the employer to fire the worker. If the employment relationship ends earlier than expected, then an employer who offered a signing bonus incurs greater costs than an employer who did not offer the bonus since the bonus is paid at the outset of the employment relationship.⁴ With respect to worker

¹ Employers may offer a signing bonus for various reasons, such as in order to hold down salaries to avoid incurring higher, on-going fixed labor costs. In addition, employers may opt to offer a signing bonus in order to avoid any adverse consequences stemming from potential salary inequities among employees (Uchitelle 1998).

² Agency theory highlights the importance of worker recruitment. The principal's task is to design a contract that motivates the agent to take desirable actions while ensuring that it also offers an expected level of utility at least as great as the agent's reservation utility.

³ Recent analytic research also demonstrates that signing bonus offers can increase worker effort (Arya et al. 2003; Van Wesep 2010). However, these studies emphasize the workers' marginal productivity of effort as a mechanism by which signing bonus offers affect effort, and do not examine the role of trust. In order to rule out this alternative explanation, I employ experimental labor markets wherein signing bonus offers have no effect on the marginal productivity of effort. Thus, my study complements extant research by highlighting an alternative factor through which signing bonus offers can positively affect worker effort.

⁴ To mitigate some of the risk associated with offering a signing bonus, some employers attach provisions requiring workers to repay some or all of the bonus if they quit or are fired for cause before some pre-specified period of time (Uchitelle 1998; Poe 1999; WorldatWork 2009). Generally, however, workers need to be employed for only a short period of time, such as six months or one year, before these repayment provisions expire. Thus, if the employer expects the employment relationship to last even a modest length of time, then offering a signing bonus, even with provisions, is risky.

effort, reciprocity leads to trustworthy behaviors, resulting in greater worker effort than if the bonus was not offered (Salamon and Robinson 2008).

Importantly, a positive effect of a signing bonus offer on effort hinges on workers' attributions. That is, workers must believe that the employer trusts them to a greater extent when they are offered a signing bonus than when they are not. In this study, I investigate how an important environmental factor—labor market competition—affects workers' attributions concerning a signing bonus offer. In the following subsection, I discuss the relevant literature on attributions, and develop hypotheses regarding the influence of labor market competition on the link between signing bonus offers and worker effort.

Workers' Attributions and Labor Market Competition

When seeking to explain the behavior of others, individuals routinely make attributions of beliefs and intentions. They rely on these attributions to determine their own behavior (Rabin 1993; Blount 1995; Falk, Fehr, and Fischbacher 2003; Falk et al. 2008; Dufwenberg and Kirchsteiger 2004; Falk and Fischbacher 2006). I examine whether labor market competition reflected in an excess demand for labor or an excess supply of labor influences workers' attributions for a signing bonus offer. Competition is a ubiquitous feature of markets, and has a considerable impact on behavior. One notable effect of market competition is the tendency for market participants to engage in a bidding war in response to competitive pressures (Roth et al. 1991; Fehr and Falk 1999; Fischbacher, Fong, and Fehr 2009). For example, Roth et al. (1991) study how competition affects behavior in the ultimatum game by manipulating whether first movers compete with other first movers to have their proposals accepted by a single second mover.⁵ They find that first movers propose divisions corresponding roughly to an equal split of a fixed amount of money when they do not face any competition from other first movers. However, when first movers compete with other first movers, they propose divisions that transfer nearly the entire sum to the second mover.

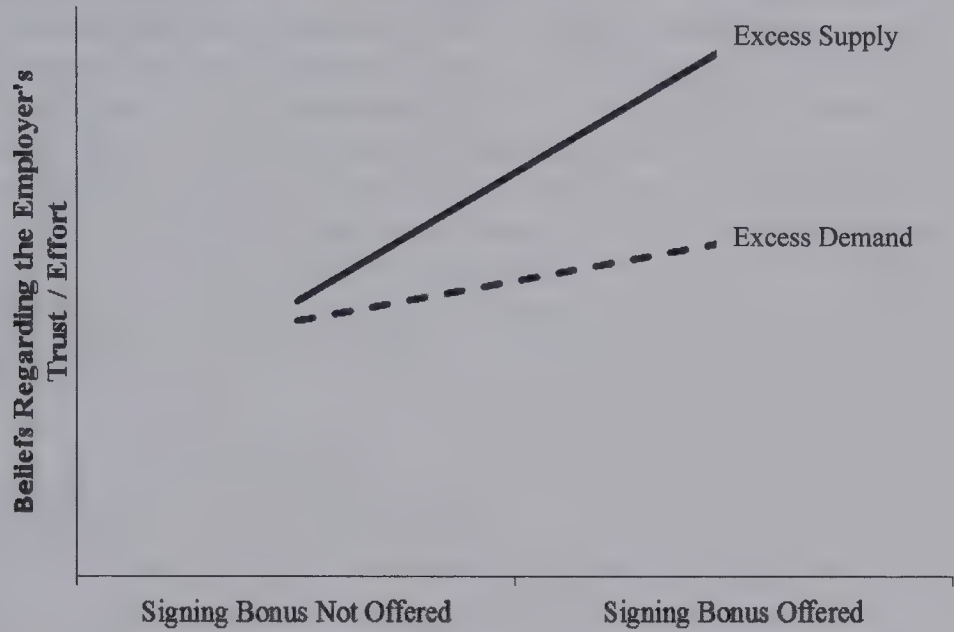
When there is an excess demand for labor, employers often respond by offering signing bonuses (Allen 1997; Uchitelle 1998; Poe 1999; WorldatWork 2007; Goolsby and Unmuth 2008; Miller 2008). Thus, workers are more likely to receive a signing bonus when there is excess demand for labor than when there is excess supply.⁶ Importantly, the greater competitive pressure that employers face when there is an excess demand for labor offers an alternative explanation for a signing bonus offer. In particular, while workers may attribute a signing bonus offer to the employer's trust in them in both labor market competition conditions, when there is an excess demand for labor, workers may also attribute the signing bonus offer to the employer's desire to avoid getting shut out of the market (Bauernschuster et al. 2013). This attribution does not apply when there is an excess supply of labor. Therefore, more attributions for the signing bonus offer are possible when there is excess demand than when there is excess supply. The discounting principle of attribution theory asserts that individuals are less likely to attribute others' behavior to any single cause when multiple plausible causes for the observed behavior exist (Heider 1958; Kelley 1973; Kelley and Michela 1980). The discounting principle suggests that workers are less likely to

⁵ The ultimatum game is a two-person, sequential move game in which the first mover proposes a division of a fixed amount of money, and the second mover decides whether to accept the first mover's proposal. If the proposal is accepted, then each player earns a payoff corresponding to the accepted proposal. If the proposal is rejected, then both players earn their reservation payoffs, usually 0.

⁶ Despite a reduced need for employers to offer a signing bonus, such offers are unlikely to completely disappear when there is excess supply. Labor market competition conditions often track broader macroeconomic conditions, in that excess demand for labor characterizes economic expansion, while excess supply characterizes economic recession. Anecdotal evidence suggests that while signing bonus offers are less prevalent during times of economic recession, they are not completely absent during those periods (Medland 2003; Van Wesep 2010).

FIGURE 1

Predicted Effects of Offering a Signing Bonus and Labor Market Competition on Workers’ Beliefs Regarding the Employer’s Trust in Them (H1) and Effort (H2)



This figure depicts the hypothesized pattern of workers’ beliefs regarding the employer’s trust in them and worker effort.

H1 predicts that the effect of offering a signing bonus on workers’ beliefs regarding the employer’s trust in them is greater when there is an excess supply of labor than when there is an excess demand for labor.

H2 predicts that the effect of offering a signing bonus on worker effort is greater when there is an excess supply of labor than when there is an excess demand for labor.

attribute the signing bonus offer to the employer’s trust in them when there is excess demand than when there is excess supply. Thus, to the extent that individuals base their own behaviors on their attributions, the positive effects of offering a signing bonus on workers’ beliefs regarding the employer’s trust in them and their corresponding effort will be greater when there is an excess supply of labor than when there is an excess demand for labor.

In summary, Hypotheses 1 and 2 predict that the effects of offering a signing bonus will depend on labor market competition, where the hypotheses are stated in alternative form and graphically presented in Figure 1:

- H1:** Signing bonus offers positively affect workers’ beliefs regarding the employer’s trust in them to a greater extent when there is an excess supply of labor than when there is an excess demand for labor.
- H2:** Signing bonus offers positively affect worker effort to a greater extent when there is an excess supply of labor than when there is an excess demand for labor.

Intertemporal Effects of Trust and Reciprocity

To the extent that signing bonuses represent an accelerated payment of compensation that would otherwise be paid in the future, testing H1 and H2 involves using a multi-period setting in which employers and workers can potentially interact repeatedly. Using such a setting introduces

additional possibilities that relate to the effects of offering a signing bonus on worker effort. For example, in a multi-period setting, there are factors that affect workers' effort choices that are not present in a single-period setting. Specifically, in contrast to the hypotheses, which emphasize workers' backward-looking attributions for employers' contract choice and their decisions regarding offering a signing bonus, workers' effort choices may also reflect their attempts to obtain future benefits such as retention, promotions, and bonuses (Fehr, Gächter, and Kirchsteiger 1997; Andreoni, Harbaugh, and Vesterlund 2003). The presence of such forward-looking concerns, however, works against finding support for the hypotheses. Specifically, given the characterization of signing bonuses above, more future compensation is at risk for workers not receiving a signing bonus. Therefore, these workers may work harder to increase their chances of getting that greater future compensation, which is counter to what the hypotheses predict.⁷

Using a multi-period setting also raises the issue of whether the effects of offering a signing bonus on effort persist over time. For example, to the extent that offering a signing bonus positively affects effort, does the employer continue to trust the worker in subsequent periods? Likewise, does the worker continue to reciprocate the employer's trust in subsequent periods? Although examining the intertemporal effects of trust and reciprocity is not the primary focus of my study, the experimental design, described next, allows me to explore this issue. I present related analyses in Section IV.

III. METHOD

Overview

To test H1 and H2, I conduct experimental labor markets in which employers hire a single worker to provide effort. Participants are randomly assigned to the role of either employer or worker, and maintain their roles throughout the experimental session. Employers decide whether to offer workers a contract, and workers decide whether to accept these contract offers. Hired workers then provide effort for up to two periods. Exerting effort positively affects the employer's payoffs, but is costly to the worker. After the employer observes the worker's effort in the first period, the employer decides whether to retain the worker for the second period. Retained workers then exert effort in the second period. Participants are informed that the second period is the last period. All interactions take place anonymously via computer. Participants' decisions affect their payoffs, which are measured in Lira, an experimental currency. At the end of the experiment, the Lira that participants earn during the session is converted to U.S. dollars at a rate of 20 Lira per \$1 and is paid to participants along with a \$5 show-up payment.

Testing the hypotheses requires comparing the situation when the signing bonus is offered to the situation when it is not across the two labor market competition conditions. I employ a 2×2 fully crossed experimental design in which I manipulate labor market competition by varying whether labor markets include two employers and one worker (excess demand) or one employer and two workers (excess supply).⁸ I also manipulate contract choice constraint, where employers have the option to include a signing bonus in the contract offer when contract choices are not

⁷ Research on framing effects would also suggest that a signing bonus offer will not necessarily create a positive effect on effort. Specifically, characterizing the bonus as a *signing* bonus frames it as a reward for agreeing to join the firm, and not as an investment for which greater effort is expected.

⁸ I use three person labor markets to minimize the number of participants needed to create an observation, and not to operationalize a monopoly or monopsony labor market setting. Given the payoff parameters that I use in my experiment, I expect participants' behavior in the labor market phase of my experiment to mirror behavior in labor markets with many employers and workers.

constrained. In contrast, when contract choices are constrained, employers do not have the option to offer a signing bonus.

Fully crossing the two factors yields four conditions: (1) excess demand/contract choice not constrained; (2) excess demand/contract choice constrained; (3) excess supply/contract choice not constrained; and (4) excess supply/contract choice constrained. Hereafter, I refer to these conditions as the *Excess Demand*, *Constrained Excess Demand*, *Excess Supply*, and *Constrained Excess Supply* conditions, respectively. The *Constrained Excess Demand* and *Constrained Excess Supply* conditions serve as benchmarks against which the effects of offering a signing bonus on workers' trust beliefs and their effort can be compared across the two labor market competition environments. Specifically, in the hypothesis tests, I use data from hired workers receiving a signing bonus in the *Excess Demand* and *Excess Supply* conditions to reflect the situation when the bonus is offered, and use data from hired workers in the *Constrained Excess Demand* and *Constrained Excess Supply* conditions to represent the situation when the bonus is not offered.⁹

Procedures

Each labor market begins with employers deciding whether to offer a contract. Employers in the *Excess Demand* and *Excess Supply* conditions can offer one of two contracts. One contract specifies a 175 Lira salary to be paid at the end of each of two periods and no signing bonus, while the other contract specifies a 145 Lira salary to be paid at the end of each of two periods and a 60 Lira signing bonus, which is paid at the beginning of the first period when the worker is hired. Note that the total compensation over two periods for a worker employed in both periods is 350 Lira under both contracts. So, any evidence of a positive effect of signing bonuses on effort is not driven by employers' choices to pay workers more total compensation.

In the *Excess Demand* condition, each of the two employers in the labor market makes a contract offer decision at the same time as the other employer without knowledge of that employer's decision. In the *Excess Supply* condition, the employer's contract offer is simultaneously presented to both workers in the labor market.¹⁰ Employers may also choose to not offer a contract. The *Constrained Excess Demand* and *Constrained Excess Supply* conditions are identical to the *Excess Demand* and *Excess Supply* conditions except that employers in the former two conditions choosing to offer a contract can only offer the 175 Lira salary contract. Further, the 145 Lira salary and 60 Lira signing bonus contract is not mentioned to participants in the two constrained contract choice conditions.

⁹ I do not rely solely on the *Excess Demand* and *Excess Supply* conditions to test the hypotheses because, as expected, all employers in the *Excess Demand* condition offer a signing bonus. Thus, including the *Constrained Excess Demand* and *Constrained Excess Supply* conditions in the experiment helps to generate the necessary data, in particular, observations of hired workers not offered the bonus when there is excess demand. Also, using data from the two constrained contract choice conditions to test the hypotheses is a more conservative approach. Any lack of trust signaled by the choice to not offer a signing bonus is dependent on employers making that choice (Christ 2013; Christ, Sedatole, and Towry 2012). For example, Christ et al. (2012) find that workers feel more trusted when their employer chooses to not implement a control than when the choice to not implement a control is exogenously imposed. In the current study, I find that hired workers from the *Constrained Excess Supply* condition feel marginally more trusted than those from the *Excess Supply* condition not offered a signing bonus (one-tailed $p = 0.07$), and I do not find a difference in effort across these two sets of workers (one-tailed $p = 0.37$).

¹⁰ Requiring the employer's contract offer to be simultaneously presented to both workers in the *Excess Supply* and *Constrained Excess Supply* conditions makes it more difficult to find support for the hypotheses. Specifically, if employers could choose which of the two workers in the labor market receives the contract offer, then a worker could attribute a signing bonus offer to the employer's trust in that particular worker ("the employer trusts me"), which could increase the likelihood of observing reciprocal behavior (Slonim and Garbarino 2008).

TABLE 1
Cost of Effort Schedule

Effort Level	1	2	3	4	5	6	7	8	9	10
Cost of Effort (in Lira)	0	10	20	30	40	50	60	70	80	90

This table presents the set of possible effort levels and the associated costs of effort. In each work period, workers select an effort level and the corresponding cost of effort is deducted from the workers' payoffs. The level of effort chosen by the worker is multiplied by 60 Lira and transferred to the worker's employer. For example, an effort level of 1 generates a payoff of $60 \times 1 = 60$ Lira for the employer, an effort level of 2 generates a payoff of $60 \times 2 = 120$ Lira for the employer, etc.

Two additional features of the contracts should be noted. First, although the total compensation over two periods for a worker employed in both periods is held constant at 350 Lira under both contracts, offering the signing bonus pays the worker more compensation up front (145 Lira salary + 60 Lira signing bonus = 205 Lira) than not offering the bonus (175 Lira salary). Thus, employers offering the bonus incur greater total labor costs if the employment relationship only lasts one period. Second, to better attribute any differences in effort across conditions to my independent variable, contracts do not include performance-based pay.

After employers make their contract offer decisions, workers view the contract offers on the computer screen and decide whether to accept them. In the *Excess Demand* and *Constrained Excess Demand* conditions, the worker views the contract offers from both employers simultaneously and selects one contract offer. In the *Excess Supply* and *Constrained Excess Supply* conditions, the two workers in the labor market observe the employer's contract choice, and simultaneously and independently decide whether to accept the contract offer. If only one worker accepts the contract, then that worker is hired. If both workers accept the contract offer, then one worker is randomly selected with equal probability to be hired.¹¹

Hired workers then choose an effort level for the first period. Consistent with prior experimental research on labor markets (e.g., Fehr et al. 1993, 1998; Hannan 2005; Kuang and Moser 2009), exerting effort entails selecting an effort level from a set of possible effort levels from 1 to 10 (see Table 1). Effort above the minimum level of 1 is costly for the worker, with these costs increasing in the level of effort, consistent with prior accounting research (Sprinkle 2000; Hannan, Krishnan, and Newman 2008; Tafov 2013; Hecht, Tafov, and Towry 2012).¹²

After the employer observes the worker's effort choice for the first period, the employer decides whether to retain the worker. Retained workers then determine an effort level for the second period and receive their second-period salary. In the two excess supply conditions, employers opting to not retain their worker after the first period cannot hire the other worker for the second period. Analogously, in the two excess demand conditions, workers who are not retained by their employer cannot be hired by the other employer for the second period. In all conditions, workers

¹¹ When both workers accept the contract offer, being hired is akin to winning a random lottery, and effort could potentially reflect a response to winning the lottery. Although such a response would not explain the hypothesized interaction, it would result in a simple main effect of labor market competition on effort. I do not find a main effect of labor market competition on either worker's trust beliefs or their effort (two-tailed $p > 0.15$ in all cases).

¹² Economic theory generally assumes that workers incur increasing marginal disutility from exerting effort. To simplify the decisions that workers must make, I use a cost of effort function that holds marginal disutility constant. Importantly, my operationalization of effort satisfies the criteria for a measure of effort outlined by Baiman (1982): exerting effort is a costly choice and is positively related to performance.

who are not retained do not receive their second-period salary. Allowing employers to choose whether to retain the worker serves two purposes. First, to the extent that workers believe that greater effort increases the likelihood that they are retained by their employer, empowering employers with the retention decision can motivate high effort across all conditions (Fehr et al. 1997; Andreoni et al. 2003), and particularly so in the two constrained contract choice conditions, since workers not offered a signing bonus have more to gain from being retained in the form of a second-period salary of 175 Lira versus a salary of 145 Lira for those offered the bonus. This makes it more difficult to find support for my hypotheses. Second, creating an environment in which the duration of the employment relationship is endogenously determined allows me to explore the intertemporal effects of trust and reciprocity.

Payoff Parameters

In all four conditions, reservation payoffs equal 0 Lira, so employers earn a payoff of 0 if they either do not offer a contract or if no workers accept their contract offers. Similarly, workers who do not accept a contract offer or do not receive a contract offer earn 0 Lira. Otherwise, participants' payoffs are calculated as follows:¹³

Employer's Payoff = 300 − Signing Bonus − ∑_{i=1}² Salary in Period_i + 60*∑_{i=1}² Effort in Period_i.

Worker's Payoff = Signing Bonus + ∑_{i=1}² Salary in Period_i − ∑_{i=1}² Cost of Effort in Period_i.

Several features of the payoffs parameters are crucial. First, setting reservation payoffs to 0 rather than a positive value increases the likelihood that employers will offer contracts and that workers will accept offered contracts. This is desirable because both of these scenarios are necessary to test my hypotheses. Second, the 300 Lira term in the employers' payoff function eliminates any effects of loss aversion on the employers' behavior. Prior research notes that individuals behave differently when losses can or will occur (Tversky and Kahneman 1991). In my setting, the possibility of losses may leave employers reluctant to offer the signing bonus, making it difficult to test my hypotheses. Moreover, the 300 Lira term partially mutes the extent to which a signing bonus offer signals the employer's trust. Since the 300 Lira term ensures that employers earn a profit even if they offer a signing bonus, employers offering the bonus are exposed to less relational risk than if the 300 Lira is not included. Third, prior research suggests that inequity aversion can also influence the worker's effort choices (Fehr and Schmidt 1999; Bolton and Ockenfels 2000). I design the payoff parameters such that inequity aversion leads workers to exert minimal effort. In particular, the last term in the employer's payoff function represents the marginal benefit the employer receives from the worker's effort choices. By multiplying the worker's effort level by 60 Lira, effort choices above the minimum level increase payoff inequity between employers and workers in the employer's favor. Thus, effort choices above the minimum level are more likely to reflect workers' reciprocity.

Session Timeline

I first randomly assigned each participant to one of the four experimental conditions. Participants then read instructions and completed a short quiz to ensure they understood the

¹³ Participants receive a comprehensive payoff chart detailing their payoffs for all contract-effort combinations. Participants can refer to this chart throughout the experiment.

instructions, including the calculation of both employers’ and workers’ payoffs. Next, participants learned their randomly assigned roles, and interacted with each other via computer, as described above. Finally, participants completed a questionnaire eliciting demographic and process-related information and received payment.

IV. RESULTS

Participants were 201 students recruited from undergraduate business courses at a highly ranked business school in the United States. On average, participants were 20.9 years old; 55 percent were male. Participants acting as employers (workers) earned an average of \$17.86 (\$12.66).¹⁴

Employers’ Contract Offer and Workers’ Contract Offer Acceptance Decisions

Table 2 reports data pertaining to participants’ behavior in the labor market phase of the experiment. Beginning with employers’ contract offer decisions, Panel A shows that all employers in each of the four conditions opted to offer a contract, which is not surprising because employers earn 0 Lira if they do not offer a contract. Panel A further shows that every employer in the *Excess Demand* condition offered the 145 Lira salary and 60 Lira signing bonus contract. Such behavior is consistent with anecdotal evidence that employers often respond to competitive pressures by offering a signing bonus (Goolsby and Unmuth 2008; Miller 2008). In contrast, in the *Excess Supply* condition 54 percent of employers offered a signing bonus. Such behavior is inconsistent with the predicted behaviors under the assumption of preferences defined solely over wealth and leisure,¹⁵ but consistent with the presence of trust. Moreover, the pattern of signing bonus offers across the *Excess Demand* and *Excess Supply* conditions accords with both anecdotal and experimental evidence, in that such offers are more prevalent when there is an excess demand for workers, but not entirely absent when there is an excess supply of workers (Fehr and Falk 1999; Medland 2003; Van Wesep 2010). By construction of the contract choice constraint manipulation, all employers in the *Constrained Excess Demand* and *Constrained Excess Supply* conditions offered the 175 Lira salary contract.

Table 2, Panel B reports workers’ contract offer acceptance decisions. All workers in each of the four conditions accepted a contract offer. Additionally, the pattern of workers’ contract acceptance decisions exactly mirrors that of employers’ contract offer decisions.

Hypothesis 1

H1 predicts that signing bonus offers positively affect workers’ beliefs regarding the employer’s trust in them to a greater extent when there is excess labor supply than when there is excess labor demand. To test this hypothesis, I ask workers the following question: “To what extent

¹⁴ The difference in earnings between employers and workers is due to conducting the *Excess Supply* condition nearly twice as many times as the other three conditions in order to have enough observations of employers offering the signing bonus when there is an excess supply of labor. Specifically, in the *Excess Supply* condition, at least one worker in each labor market is guaranteed to earn only the \$5 show-up payment, which lowers the overall average earnings workers received for participating in the experiment.

¹⁵ Under this assumption, employers in the *Excess Supply* condition will always offer the 175 Lira salary contract. This prediction can be seen using backward induction. In the second period, workers will always exert minimal effort. Anticipating this, employers will fire the worker after the first period. Workers will anticipate employers’ firing decision and exert minimal effort in the first period. Thus, when making their contract choice, employers in the *Excess Supply* condition will expect the behavior outlined above and offer the 175 Lira salary contract because employers will incur lower labor costs than if they offer the 145 Lira salary and 60 Lira signing bonus contract.

TABLE 2

Employers' Contract Offer and Workers' Contract Acceptance Decisions

Panel A: Employers' Contract Offer Choices

Condition	Total # of Labor Markets	Total # of Employers	# of Employers Offering Contracts (% of Total)	# of Employers Offering 175 Lira Salary Contract (% of Employers Offering Contracts)	# of Employers Offering 145 Lira Salary and 60 Lira Signing Bonus Contract (% of Employers Offering Contracts)
Excess Demand ^a	15	30	30 (100%)	0 (0%)	30 (100%)
Excess Supply ^b	26	26	26 (100%)	12 (46%)	14 (54%)
Constrained Excess Demand ^c	13	26	26 (100%)	26 (100%)	0 (0%)
Constrained Excess Supply ^d	13	13	13 (100%)	13 (100%)	0 (0%)

Panel B: Workers' Contract Offer Acceptance Decisions

Condition	Total # of Labor Markets	Total # of Workers	# of Workers Accepting Contract Offers (% of total)	# of Workers Accepting 175 Lira Salary Contract (% of Workers Accepting Contracts)	# of Workers Accepting 145 Lira Salary and 60 Lira Signing Bonus Contract (% of Workers Accepting Contracts)
Excess Demand ^a	15	15	15 (100%)	0 (0%)	15 (100%)
Excess Supply ^b	26	52	52 (100%)	24 (46%)	28 (54%)
Constrained Excess Demand ^c	13	13	13 (100%)	13 (100%)	0 (0%)
Constrained Excess Supply ^d	13	26	26 (100%)	26 (100%)	0 (0%)

^a In the *Excess Demand* condition, each labor market consists of 2 employers and 1 worker. Employers can choose to offer the worker either a contract specifying a 175 Lira salary to be paid at the end of each work period or a contract specifying a 145 Lira salary and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). Employers can also choose to not offer a contract. Employers make their contract offer decisions simultaneously and independently of each other. The worker views the contract offers from both employers simultaneously and selects one contract offer. Workers may also choose to not accept any contract offers.

^b In the *Excess Supply* condition, each labor market consists of 1 employer and 2 workers. Employers can choose to offer the workers either a contract specifying a 175 Lira salary to be paid at the end of each work period or a contract specifying a 145 Lira salary to be paid at the end of each work period and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). Employers can also choose to not offer a contract. In each labor market, the two workers are shown the employer's contract offer, and they simultaneously and independently decide whether to accept the contract offer. If only one worker accepts the contract, then that worker is hired. If both workers accept the contract offer, then one worker is randomly selected to be hired. Workers may also choose to not accept any contract offers.

^c The *Constrained Excess Demand* condition is identical to the *Excess Demand* condition, except that employers choosing to offer a contract can only offer the 175 Lira salary contract.

^d The *Constrained Excess Supply* condition is identical to the *Excess Supply* condition, except that employers choosing to offer a contract can only offer the 175 Lira salary contract.

do you believe that the employer trusts you?" Responses to this question are captured using a 101-point Likert scale, where 0 = "Not at all" and 100 = "A great deal." Workers answer this question after they learn that they have been hired, and I counterbalance whether workers answer the question before or after selecting an effort level.¹⁶ Table 3, Panel A presents mean responses to this question in the column entitled "First-Period Trust," and Figure 2, Panel A provides a graphical depiction. A comparison of the predicted pattern of results in Figure 1 with the experimental results for workers' trust beliefs in Figure 2, Panel A suggests that the two patterns are similar.

Since H1 predicts an ordinal interaction, I test H1 using a planned contrast (Buckless and Ravenscroft 1990). I use contrast weights of +3 for the *Excess Supply* condition, +1 for the *Excess Demand* condition, and -2 for both the *Constrained Excess Demand* and *Constrained Excess Supply* conditions. Rosnow and Rosenthal (1995) identify these as appropriate weights for the ordinal interaction predicted by H1.¹⁷

As reported in Table 3, Panel B workers' beliefs regarding their employer's trust in them are consistent with the pattern hypothesized by H1 ($F_{1,51} = 23.90$, $p < 0.01$), supporting H1. Untabulated simple effects tests indicate that within both labor market competition environments, offering a signing bonus positively affects workers' beliefs, although marginally so when there is excess demand (excess demand one-tailed $p = 0.09$; excess supply one-tailed $p < 0.01$). Untabulated simple effects tests comparing across labor market competition environments reveal that trust beliefs are greater for workers receiving a signing bonus when there is an excess supply of labor than for workers receiving a signing bonus when there is an excess demand for labor (one-tailed $p < 0.01$). For workers in the two constrained conditions, trust beliefs are not statistically different (two-tailed $p = 0.76$). These results suggest that offering a signing bonus positively affects workers' beliefs about being trusted by the employer, but this effect is stronger when there is an excess supply of labor than when there is excess demand.

Hypothesis 2

H2 predicts that signing bonus offers positively affect worker effort more when there is excess labor supply than when there is excess labor demand. To test this hypothesis, I compare workers' first-period effort choices across conditions. Since H2 is concerned with workers' effort choices in response to being offered a signing bonus, I focus primarily on first-period effort choices and not effort across both periods, since second-period effort choices are likely to also reflect employers' decisions to retain the worker. I report mean effort levels in Table 3, Panel A and graphically depict them in Figure 2, Panel B. Comparing the actual results for effort in Figure 2, Panel B with Figure 1 suggests that the two patterns are similar.

Because H2 predicts an ordinal interaction similar to H1, I also test H2 using a planned contrast, and use the same set of contrast weights as those used to test H1. Table 3, Panel B shows

¹⁶ Since I am interested in capturing workers' beliefs in response to (not) receiving a signing bonus offer, I elicit workers' responses after they learn that they have been hired. While asking this question during the course of the experiment likely raises concerns that doing so affects workers' effort choices, recent studies examining this issue (Guerra and Zizzo 2004; Zizzo 2010) suggest that eliciting feelings and beliefs of being trusted by others does not influence subsequent behavior. Nonetheless, for both periods, I counterbalance whether workers answer the trust question before or after selecting an effort level. Results are unaffected by the order in which workers' responses and effort choices are elicited.

¹⁷ For both hypotheses, untabulated results show that the interaction term from the ANOVA model is statistically significant. However, I use a planned contrast as the primary test of the hypothesized ordinal interaction because the ANOVA model misallocates the variance attributable to the ordinal interaction between the main and interaction effects (Buckless and Ravenscroft 1990). I also test both hypotheses using contrast weights of +3 in the *Excess Supply* condition and -1 in the other three conditions, since this alternate set of contrast weights is also consistent with the ordinal interaction predicted by H1 and H2. For both hypotheses, untabulated results show that the planned contrast is statistically significant when using this alternate set of contrast weights.

TABLE 3

Tests of Hypotheses

Panel A: Descriptive Statistics—Mean (Standard Deviation) [n]

Experimental Condition ^a	Theory-Level Scenario Represented by Experimental Condition ^b	First-Period Trust ^c			First-Period Effort ^d		
<i>Excess Demand</i>	Signing Bonus Offered: <i>Excess Demand</i>	53.27	(29.14)	[15]	4.07	(2.09)	[15]
<i>Excess Supply</i>	Signing Bonus Offered: <i>Excess Supply</i>	84.79	(12.83)	[14]	5.50	(1.74)	[14]
<i>Constrained Excess Demand</i>	Signing Bonus Not Offered: <i>Excess Demand</i>	40.54	(27.89)	[13]	3.85	(2.27)	[13]
<i>Constrained Excess Supply</i>	Signing Bonus Not Offered: <i>Excess Supply</i>	43.54	(25.76)	[13]	3.54	(1.45)	[13]

Panel B: Hypothesis Tests (Planned Contrasts)^e

Source of Variation	df	MS	F	p-value (two-tailed)
Hypothesis 1—First-Period Trust Beliefs				
Model Contrast	1	14,681.67	23.90	< 0.01
Error	51	614.23		
Hypothesis 2—First-Period Effort				
Model Contrast	1	25.37	6.91	0.01
Error	51	3.67		

^a In the *Excess Demand* condition, labor markets consist of 2 employers and 1 worker. Employers can offer a contract specifying either a 175 Lira salary or a 145 Lira salary and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). Employers can also choose to not offer a contract. Employers make their contract offer decisions simultaneously and independently of each other. The worker views the contract offers from both employers simultaneously and selects one contract offer. Workers may also choose to not accept any contract offers. In the *Excess Supply* condition, labor markets consist of 1 employer and 2 workers. Employers can offer a contract specifying either a 175 Lira salary or a 145 Lira salary and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). In each labor market, the employer’s contract offer is a “public” offer; the employer’s contract offer is presented to both workers in the labor market. Employers can also choose to not offer a contract. In each labor market, the two workers are shown the employer’s contract offer, and they simultaneously and independently decide whether to accept the contract offer. If only one worker accepts the contract, then that worker is hired. If both workers accept the contract offer, then one worker is randomly selected to be hired. Workers may also choose to not accept any contract offers. The *Constrained Excess Demand* (*Constrained Excess Supply*) condition is identical to the *Excess Demand* (*Excess Supply*) condition, except that employers can only offer the 175 Lira salary contract.

^b To capture the situation when the signing bonus is (not) offered across the two labor market competition environments, I use data from hired workers accepting the 145 Lira salary and 60 Lira signing bonus (175 Lira salary) contract in the (*Constrained*) *Excess Demand* and (*Constrained*) *Excess Supply* conditions.

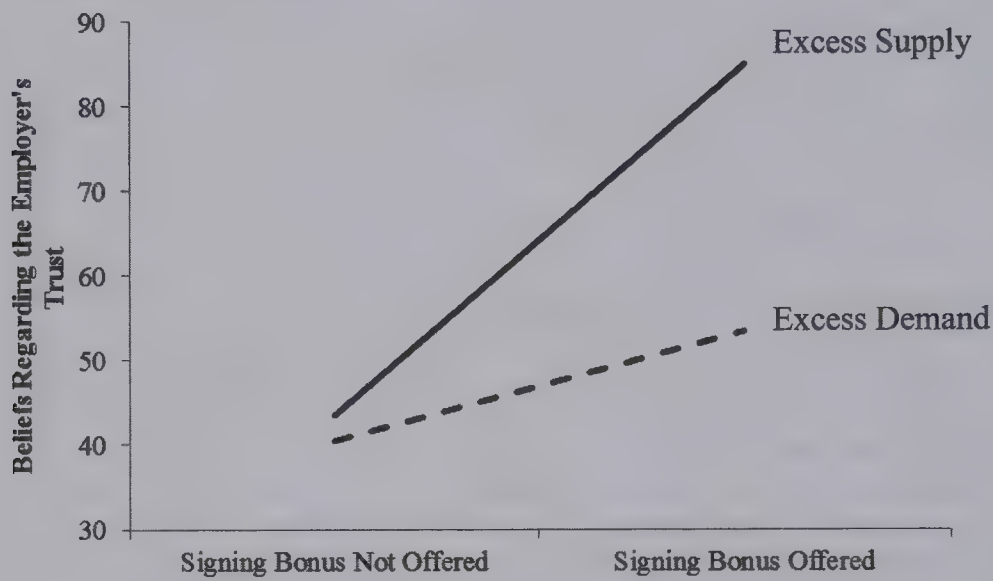
^c Trust is measured using hired workers’ responses to the following question: “To what extent do you believe that the employer trusts you?” Responses to this question are captured using a 101-point Likert scale, where 0 = “Not at all” and 100 = “A great deal.” Hired workers answer this question after being hired, either before or after choosing their first-period effort (counterbalanced in each condition).

^d Effort captures hired workers’ selected effort level from a set of possible effort levels. The lowest possible effort level is 1, and the highest possible effort level is 10. Effort above the minimum level is costly for the worker, with these costs increasing in the level of effort chosen (see Table 1).

^e Contrast coefficients are −2 for both the *Constrained Excess Demand* and *Constrained Excess Supply* conditions, +1 for the *Excess Demand* condition, and +3 for the *Excess Supply* condition.

FIGURE 2
Experimental Results of Offering a Signing Bonus^a and Labor Market Competition^b on Workers' First-Period Trust Beliefs^c (H1) and Effort^d (H2)

Panel A: First-Period Trust Beliefs



(continued on next page)

that the pattern of worker effort is consistent with that predicted by H2 ($F_{1,51} = 6.91, p = 0.01$). Thus, H2 is supported. Untabulated simple effects tests indicate that when there is an excess demand for labor, signing bonus offers do not positively affect worker effort (one-tailed $p = 0.38$). In contrast, when there is an excess supply of labor, offering a signing bonus does positively affect worker effort ($p < 0.01$, one-tailed). Untabulated simple effects tests comparing across labor market competition environments reveal that workers receiving a signing bonus exert greater effort when there is an excess supply of labor than when there is an excess demand for labor (one-tailed $p = 0.03$). For hired workers in the two constrained conditions, effort is not statistically different across competition environments (two-tailed $p = 0.68$).

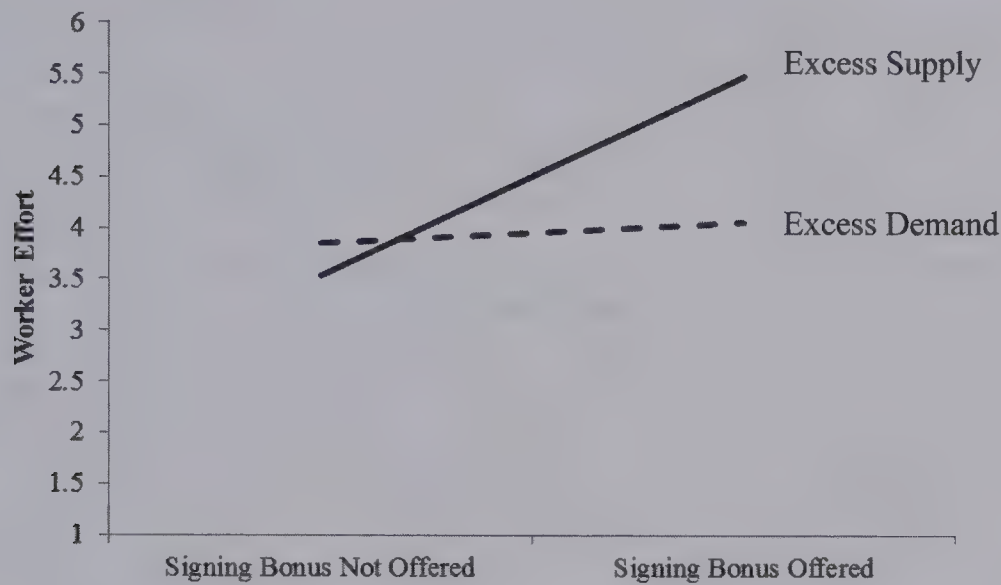
To assess whether the results above extend to effort over both periods, I also test H2 using total effort as the dependent variable. In these tests, I measure total effort in three ways: (1) replacing fired workers' missing second-period effort choice with effort level 0; (2) replacing fired workers' missing second-period effort choice with effort level 1; and (3) using only retained workers. For all three measures, I find marginal support for H2; for replace with 0, $F_{1,51} = 3.34, p = 0.07$; for replace with 1, $F_{1,51} = 3.03, p = 0.08$; for retained workers only, $F_{1,31} = 2.94, p = 0.09$. These results suggest that offering a signing bonus positively affects both initial and total effort, with the effect being greater when there is an excess supply of labor.

Path Analysis

To further analyze the role of trust as the mechanism by which signing bonus offers positively affect worker effort, I use AMOS software to perform a path analysis that estimates the links among signing bonus offers, workers' trust beliefs, and effort. Figure 3 presents the path analysis model and the corresponding results. Although all links are estimated simultaneously, I employ multi-group analysis and report separate estimates of the link between offering a signing bonus and

FIGURE 2 (continued)

Panel B: First-Period Effort



^a Signing bonus is measured at two levels, offered and not offered. Data reflecting the situation in which the signing bonus is (not) offered comes from the (*Constrained*) *Excess Demand* and (*Constrained*) *Excess Supply* conditions.

^b Labor market competition is manipulated as a between-subjects factor at two levels, excess demand and excess supply. Labor markets characterized by excess demand (excess supply) consist of 2 employers (1 employer) and 1 worker (2 workers).

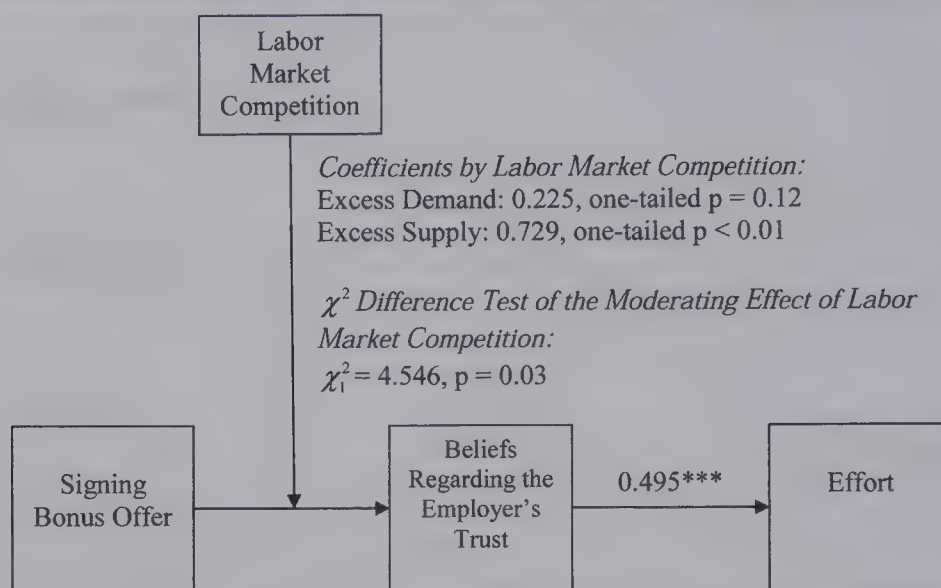
^c Trust is measured using hired workers’ responses to the following question: “To what extent do you believe that the employer trusts you?” Responses to this question are captured using a 101-point Likert scale, where 0 = “Not at all” and 100 = “A great deal.” Hired workers answer this question after being hired, either before or after choosing their first-period effort (counterbalanced in each condition).

^d Effort captures hired workers’ selected effort level from a set of possible effort levels. The lowest possible effort level is 1, and the highest possible effort level is 10. Effort above the minimum level is costly for the worker, with these costs increasing in the level of effort chosen (see Table 1).

workers’ trust beliefs for the two labor market competition environments. Since I do not expect the link between workers’ trust beliefs and effort to be differentially affected by whether there is excess demand or excess supply, I restrict the estimate of this link to be identical across labor market competition conditions. A χ^2 test confirms the model’s goodness of fit ($\chi^2_3 = 2.04$, $p = 0.56$). The model’s fit is further confirmed by the $\chi^2/\text{degrees of freedom}$ (0.68), which is less than the recommended maximum of 5, the comparative fit index (1.00), which exceeds the recommended minimum of 0.95, and the root mean square effort of approximation (0.00), which is below the recommended maximum of 0.05 (Byrne 2001; Fan, Thompson, and Wang 1999; Hu and Bentler 1999).

As expected, there is a strong positive statistical association between workers’ beliefs regarding the employer’s trust in them and their effort (path coefficient = 0.495, one-tailed $p < 0.01$). The estimates of the link between offering a signing bonus and workers’ trust beliefs across labor market competition conditions are also consistent with expectations. Specifically, when there is an excess supply of labor, I find a strong positive association between offering a signing bonus and workers’

FIGURE 3
Path Analysis Results of Estimated Links among Theoretical Constructs



*** Indicates significance at a level of 0.01 (one-tailed).

This figure reports the results of a multi-group path analysis estimating the links among signing bonus offers, workers' beliefs regarding the employer's trust in them, and effort. The standardized path coefficients and corresponding one-tailed significance are shown for each path. Goodness of fit is evaluated with a χ^2 test ($\chi^2 = 2.04, p = 0.56$), and confirmed with three other goodness-of-fit measures: the $\chi^2/\text{degrees of freedom}$ ($\chi^2/\text{df} = 0.68$; values < 5 are considered adequate fit), the comparative fit index ($\text{CFI} = 1.00$; values > 0.95 are considered adequate fit), and the root mean error of approximation ($\text{RMSEA} = 0.00$; values < 0.05 are considered adequate fit).

trust beliefs (path coefficient = 0.729, one-tailed $p < 0.01$). In contrast, when there is an excess demand for labor, I do not find a statistical association between offering a signing bonus and workers' beliefs (path coefficient = 0.225, one-tailed $p = 0.12$).

To confirm the interaction suggested by the path analysis results reported above, I conduct a χ^2 difference test. The null hypothesis of this test is that the effect of signing bonus offers on workers' trust beliefs does not depend on labor market competition; i.e., that there is no interaction, while the alternative hypothesis is that there is an interaction between signing bonus offers and labor market competition. Based on the results of the χ^2 difference test, I reject the null in favor of the alternative ($\chi^2_1 = 4.572, p = 0.03$). On the whole, the path analysis results corroborate the findings of my hypothesis tests. That is, offering a signing bonus positively affects worker effort, with this effect being stronger when there is excess supply than when there is excess demand. Further, the impact of signing bonus offers on workers' beliefs regarding the employer's trust in them drives this effect. That is, workers attribute a signing bonus offer to the employer's trust in them to a greater extent when there is an excess supply of labor than when there is an excess demand for labor.

Intertemporal Effects of Trust and Reciprocity

Using my experimental design, I next explore whether the interactive effects of signing bonus offers and labor market competition persist over time. Specifically, I examine whether the effect of offering a signing bonus on workers' first-period effort across labor market competition conditions influences whether employers and workers engage in a gift exchange in the second period. The

potential for gift exchange in the second period arises because employees who retain the worker must pay the second-period salary (either 145 Lira or 175 Lira, depending on the condition), but earn a positive payoff in the second period only if the retained worker reciprocates the employer's trust with an effort level of at least 3, which is above the minimum effort level.

In this subsection, I examine two related questions. First, does the effect of offering a signing bonus on workers' first-period effort influence employers' retention decisions? That is, do retention rates differ across conditions? Second, does being retained affect workers' subsequent trust beliefs and effort? Before discussing the analyses, it is important to note that cell sizes are relatively small at 7 to 10 observations per condition (see Table 4). Thus, the analyses reported below should be interpreted with caution.

I first examine whether retention rates differ across conditions. Retention decisions likely depend on employers' expectations of second-period effort, where these expectations likely depend on workers' first-period effort. Although first-period effort differs across conditions, whether this leads to differences in retention is unclear. One possibility is that retention is higher in the *Excess Supply* condition than in the other three conditions, since first-period effort more clearly reflects the worker's reciprocity in that condition than in the other three conditions. Another possibility is that retention decisions are based on whether workers fulfill their employers' expectations of first-period effort, with greater retention when workers fulfill those expectations. In this case, whether differences in first-period effort lead to differences in retention is unclear. Specifically, offering a signing bonus is a trusting action grounded in employers' expectations of effort. Therefore, employers who offer the bonus likely expect greater first-period effort than those not offering the bonus, especially when there is an excess supply of labor. *Ex ante*, however, there is no reason to expect differences in the extent to which workers fulfill their employer's first-period effort expectations across conditions.

Table 4, Panel A reports that at least half of the hired workers in each condition are retained for the second period, with the percentage retained ranging from 54 to 71 percent. Further, Table 4, Panel B shows that retention decisions do not differ across conditions ($\chi^2_1 = 0.74$, $p = 0.39$). Given the discussion above, I also compare employers' expectations of first-period effort to workers' actual first-period effort choices, and then examine how this relation affects retention decisions. Before learning their worker's first-period effort choice, employers indicate how much effort they expect the worker to exert. I elicit these expectations concurrently with workers' trust beliefs and effort choices.¹⁸ Untabulated results show that employers expect much greater effort from their workers in the *Excess Supply* condition than in the other three conditions ($F_{3,51} = 47.62$, $p < 0.01$). On average, employers in the *Excess Supply* condition expect an effort level of 6.00, while effort expectations in the other three conditions range from 2.38 to 2.62. Workers are also less likely to fulfill their employer's first-period effort expectations in the *Excess Supply* condition than in the other conditions ($\chi^2_3 = 18.51$, $p < 0.01$). Across conditions, whether workers fulfill their employer's first-period effort expectations appears to be an important factor for retention, as a majority of workers who meet or exceed their employers' effort expectations were retained for the second period ($\chi^2_3 = 6.33$, $p = 0.01$). These results suggest that fulfillment of prior period effort expectations may play an important role in whether employers initiate a gift exchange in the second period.

I next examine whether being retained affects workers' subsequent trust beliefs and effort. Unlike the hiring decision, there is no market environment that moderates the retention decision; workers hired in the first period are either retained or the relationship ends and both parties earn 0 Lira. Thus, if retention signals trust, then not only could second-period effort be above the minimum level, but also retention may lead to similar levels of second-period trust beliefs and effort

¹⁸ These expectations were not communicated to the worker.

TABLE 4
Intertemporal Effects of Trust and Reciprocity

Panel A: Descriptive Statistics—Mean (Standard Deviation) [n]					
Experimental Condition ^a	Theory-Level Scenario Represented by Experimental Condition ^b	# of Employment Relationships Initiated	# of Workers Retained for Second Period (% Retained)	Second-Period Trust ^c	Second-Period Effort ^d
Excess Demand	Signing Bonus Offered: Excess Demand	15	9 (60%)	60.22 (30.39) [9]	2.00 (2.35) [9]
Excess Supply				66.80 (16.57) [10]	2.40 (1.59) [10]
Constrained Excess Demand	Signing Bonus Not Offered: Excess Demand	13	9 (69%)	58.11 (31.01) [9]	3.44 (2.83) [9]
Constrained Excess Supply	Signing Bonus Not Offered: Excess Supply	13	7 (54%)	52.14 (27.21) [7]	2.43 (1.51) [7]

Panel B: ANOVA Results

Dependent Measure	Test Statistic	p-value (two-tailed)
Proportion of Retained Workers	$\chi^2_1 = 0.74$	0.39
Workers' Second-Period Trust Beliefs	F = 0.84	0.48
Workers' Second-Period Effort	F = 0.73	0.54

(continued on next page)

TABLE 4 (continued)

^a In the <i>Excess Demand</i> condition, labor markets consist of 2 employers and 1 worker. Employers can offer a contract specifying either a 175 Lira salary or a 145 Lira salary and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). Employers can also choose to not offer a contract. Employers make their contract offer decisions simultaneously and independently of each other. The worker views the contract offers from both employers simultaneously and selects one contract offer. Workers may also choose to not accept any contract offers. In the <i>Excess Supply</i> condition, labor markets consist of 1 employer and 2 workers. Employers can offer a contract specifying either a 175 Lira salary or a 145 Lira salary and a 60 Lira signing bonus, which is paid at the beginning of the first work period (i.e., when the worker is hired). Employers can also choose to not offer a contract. In each labor market, the employer's contract offer is a "public" offer; the employer's contract offer is presented to both workers in the labor market. In each labor market, the two workers are shown the employer's contract offer, and they simultaneously and independently decide whether to accept the contract offer. If only one worker accepts the contract, then that worker is hired. If both workers accept the contract offer, then one worker is randomly selected to be hired. Workers may also choose to not accept any contract offers. The <i>Constrained Excess Supply</i> condition is identical to the <i>Excess Demand</i> (<i>Excess Supply</i>) condition, except that employers can only offer the 175 Lira salary contract. ^b To capture the situation when the signing bonus is (not) offered across the two labor market competition environments, I use data from hired workers accepting the 145 Lira salary and 60 Lira signing bonus (175 Lira salary) contract in the (<i>Constrained</i>) <i>Excess Demand</i> and (<i>Constrained</i>) <i>Excess Supply</i> conditions. ^c Trust is measured using hired workers' responses to the following question: "To what extent do you believe that the employer trusts you?" Responses to this question are captured using a 101-point Likert scale, where 0 = "Not at all" and 100 = "A great deal." Hired workers answer this question after being hired, either before or after choosing their first-period effort (counterbalanced in each condition). ^d Effort captures hired workers' selected effort level from a set of possible effort levels. The lowest possible effort level is 1, and the highest possible effort level is 10. Effort above the minimum level is costly for the worker, with these costs increasing in the level of effort chosen (see Table 1).
--

across conditions since second-period salaries are relatively similar across conditions. In addition, since the second period is a continuation of the employment relationship, retention could lead to changes in trust beliefs and effort across periods.

I measure workers' second-period trust beliefs and effort using similar procedures to those in the first period. After workers learn that they are retained for the second period, they respond to the same question I used in the first period to elicit trust beliefs, and I counterbalance whether workers answer the question before or after exerting effort. Table 4, Panel A shows that average trust beliefs across conditions range from 52.14 to 66.80, and that average effort across conditions range from 2.00 to 2.43. In addition, Table 4, Panel B, shows that neither second-period trust beliefs nor effort choices differ across conditions (*beliefs*: $F_{3,31} = 0.84$, $p = 0.48$; *effort*: $F_{3,31} = 0.73$, $p = 0.54$).¹⁹ Untabulated analyses also indicate that effort is greater than the minimum level in three of the four conditions, even though workers knew that the employment relationship would end after the second period (two-tailed $p = 0.24$ for the *Excess Demand* condition, and two-tailed $p \leq 0.05$ for the other three conditions).

Untabulated analyses also suggest that being retained can lead to changes in retained workers' trust beliefs and effort across periods. Retained workers' trust beliefs do not differ across periods in the *Excess Supply* condition, although the difference is marginally insignificant (two-tailed $p = 0.15$). In the other three conditions, workers feel marginally more trusted after being retained (two-tailed $p \leq 0.09$). In terms of effort, retained workers in all four conditions exert lower effort in the second period than in the first period, although the decrease is not statistically significant in the *Constrained Excess Demand* condition (two-tailed p 's ≤ 0.03 except for *Constrained Excess Demand*, two-tailed $p = 0.23$).

One potential explanation for these results is that workers' expectations of being retained were higher in the *Excess Supply* condition than in the other three conditions. Since signing bonus offers more clearly signal trust when there is excess supply than when there is excess demand, workers likely viewed their employer as being more inherently trusting in the *Excess Supply* condition than in the other three conditions (Fehr and Schmidt 2007; Rigdon 2009). In addition, given the pattern of first-period effort, workers were also likely to be more inclined to believe that their employer would develop favorable expectations of their trustworthiness in the second period in the *Excess Supply* condition. Thus, workers in the *Excess Supply* condition may have not viewed being retained as conveying more trust, but still reciprocated their employer's continuing high levels of trust with effort above the minimum level. In contrast, given their lower trust beliefs and expectations of being retained, workers in the other conditions may have felt more trusted after being retained, where this decision more clearly signals the employer's trust than the initial contract offer, and reciprocated with effort above the minimum level.

Taken together, the results suggest that both employers' and workers' expectations may affect whether and how trust and reciprocity develop over time (Kramer 1999; Kuang and Moser 2011). As noted earlier, however, caution is warranted in interpreting the results above due to small cell sizes. Thus, our understanding of the intertemporal effects of trust and reciprocity would benefit from additional research.

¹⁹ As in the first period, trust beliefs and effort are positively related in the second period ($r = 0.42$, two-tailed $p = 0.01$). However, since total compensation is held constant at 350 Lira, the second-period salary is lower in the two unconstrained conditions (145 Lira) than in the two constrained conditions (175 Lira). Thus, the positive association between second-period trust beliefs and effort could simply reflect differences in second-period pay. If so, then I would observe a main effect of receiving a signing bonus on second-period trust beliefs and effort. However, untabulated ANOVA results indicate no statistically significant main effect (two-tailed $p > 0.24$ in both cases).

V. CONCLUSION

This study investigates whether labor market competition moderates the effects of signing bonuses on worker effort. I find support for this expectation because signing bonus offers positively affect effort to a greater extent when there is an excess supply of labor than when there is an excess demand for labor. I also find that the impact of signing bonus offers on effort operates through workers' beliefs about the employer's trust in them. I also find that the initial effects of signing bonus offers may not persist over time. Additional analyses suggest that employers' and workers' expectations may affect how trust and reciprocity develop over time.

My study contributes to the literature on the use of trust as an informal control mechanism and the related phenomenon of gift exchange. In particular, I find that labor market competition is one factor that affects whether employers and workers can successfully consummate a gift exchange. In a related study, Brandts and Charness (2004) examine the effects of labor market competition on wage offers and effort in experimental labor markets. They report only modest evidence of higher wage offers when there is excess demand for labor than when there is excess supply of labor, and no differences in effort across competition conditions. However, their experimental design includes reservation payoffs that discourage employers from making wage offers or bidding up wages. Consequently, employers do not face any competitive pressure from other employers even when there is excess demand, and workers are unlikely to make differential attributions for wage offers across labor market competition conditions. In contrast, my experimental design includes payoff parameters that induce competition among employers when there is an excess demand for labor. Thus, my results offer insights concerning the conditions under which employers are better able to make use of trust and reciprocity in motivating organizationally desirable behavior from their workers, and highlight the moderating effects of the contextual environment on workers' responses to their employer's actions (Sen 1997; Bowles 1998; Falk et al. 2006; Kuang and Moser 2009).

My study also contributes to the extant literature on signing bonuses. Recent studies go beyond the oft-cited benefit of improved worker recruitment and demonstrate that signing bonus offers can increase worker effort (Arya et al. 2003; Van Wesep 2010). These studies, however, emphasize the workers' marginal productivity of effort as a mechanism by which signing bonuses affect effort. My study complements prior research by showing that a signing bonus can positively affect worker effort by conveying trust and appealing to the worker's reciprocity.

The limitations of the current study can be addressed by future research. For example, both academics and practitioners would benefit from additional, more direct, evidence on the role that expectations play in how the effects of trust and reciprocity develop over time. More generally, future research could help identify factors that influence the exchange of trusting and reciprocal actions in multi-period settings. With regard to the effort-inducing properties of signing bonuses, future research could examine how attaching payback provisions affect workers' trust beliefs and their effort. Future studies could also investigate how the choice to offer a signing bonus affects the decision to include incentive pay as well as the weight placed on performance measures. Another interesting avenue for future research involves contract negotiation (Kuang and Moser 2011). Since employee compensation is often negotiated, insights regarding the effects of negotiation on employers' propensity to offer a signing bonus, as well as on the link between signing bonuses and effort would be beneficial. Finally, future research could explore the effects of information asymmetry on the signing bonus-effort relationship. Employers often have better information about a worker's reputation relative to other workers in the labor market. When a signing bonus is offered, workers may attribute the offer to the employer's information, and subsequently believe that their reputation is relatively positive. Research suggests that such beliefs will positively affect the worker's intrinsic motivation, leading to an even greater effect of signing bonus offers on worker

effort (Rosenfeld, Folger, and Adelman 1980; Sansone 1986). Future research could investigate this signaling role of signing bonus offers.

REFERENCES

- Akerlof, G. A. 1982. Labor contracts as partial gift exchange. *The Quarterly Journal of Economics* 97 (4): 543–569.
- Allen, C. 1997. Signing bonuses. *Journal of Career Planning & Employment* 57: 29–32.
- Andreoni, J., W. Harbaugh, and L. Vesterlund. 2003. The carrot or the stick: Rewards, punishments, and cooperation. *American Economic Review* 93 (3): 893–902.
- Arrow, K. J. 1970. Political and economic evaluation of social effects and externalities. In *The Analysis of Public Output*, edited by J. Margolis, 1–30. Ann Arbor, MI: UMI.
- Arya, A., H. Frimor, and B. Mittendorf. 2003. Pouring money down the drain? How sunk investments and signing bonuses can improve employee incentives. *Topics in Economic Analysis & Policy* 3: Article 6.
- Bacharach, M., G. Guerra, and D. J. Zizzo. 2007. The self-fulfilling property of trust: An experimental study. *Theory and Decision* 63: 349–388.
- Baiman, S. 1982. Agency research in managerial accounting: A survey. *Journal of Accounting Literature* 1: 154–213.
- Bauernschuster, S., O. Falck, and N. Große. 2013. When trustors compete for the favour of a trustee—A laboratory experiment. *Journal of Economic Psychology* 34: 133–147.
- Berg, J., J. Dickhaut, and K. McCabe. 1995. Trust, reciprocity, and social history. *Games and Economic Behavior* 10: 122–142.
- Blount, S. 1995. When social outcomes aren't fair: The effect of causal attributions on preferences. *Organizational Behavior and Human Decision Processes* 63: 131–144.
- Bolton, G. E., and A. Ockenfels. 2000. ERC: A theory of equity, reciprocity, and competition. *American Economic Review* 90: 166–193.
- Bowles, S. 1998. Endogenous preferences: The cultural consequences of markets and other economic institutions. *Journal of Economic Literature* 36: 75–111.
- Brandts, J., and G. Charness. 2004. Do labor market conditions affect gift exchange? Some experimental evidence. *The Economic Journal* 114: 684–708.
- Buckless, F. A., and S. P. Ravenscroft. 1990. Contrast coding: A refinement of ANOVA in behavioral analysis. *The Accounting Review* 65: 933–945.
- Byrne, B. M. 2001. *Structural Equation Modeling with AMOS: Basic Concepts, Applications, and Programming*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Christ, M. H. 2013. An experimental investigation of the interactions among intentions, reciprocity and control. *Journal of Management Accounting Research* 25 (1): 169–197.
- Christ, M. H., K. L. Sedatole, and K. L. Towry. 2012. Sticks and carrots: The effect of control frame on effort in incomplete contracts. *The Accounting Review* 87 (6): 1913–1938.
- Cox, J. C. 2004. How to identify trust and reciprocity. *Games and Economic Behavior* 46: 260–281.
- Das, T. K., and B. Teng. 1998. Between trust and control: Developing confidence in partner cooperation in alliances. *Academy of Management Review* 23: 491–512.
- Das, T. K., and B. Teng. 2001. Trust, control, and risk in strategic alliances: An integrated framework. *Organization Studies* 22: 251–283.
- Dohmen, T., A. Falk, D. Huffman, and U. Sunde. 2009. Homo Reciprocans: Survey evidence on behavioral outcomes. *The Economic Journal* 119: 592–612.
- Dufwenberg, M., and G. Kirchsteiger. 2004. A theory of sequential reciprocity. *Games and Economic Behavior* 47: 268–298.
- Falk, A., and U. Fischbacher. 2006. A theory of reciprocity. *Games and Economic Behavior* 54: 293–315.
- Falk, A., E. Fehr, and C. Zehnder. 2006. Fairness perceptions and reservation wages—The behavioral effects of minimum wage laws. *Quarterly Journal of Economics* 121: 1347–1381.

- Falk, A., E. Fehr, and U. Fischbacher. 2003. On the nature of fair behavior. *Economic Inquiry* 41: 20–26.
- Falk, A., E. Fehr, and U. Fischbacher. 2008. Testing theories of fairness – Intentions matter. *Games and Economic Behavior* 62: 287–303.
- Fan, X., B. Thompson, and L. Wang. 1999. Effects of sample size, estimation methods, and model specification on structural equation modeling fit indexes. *Structural Equation Modeling* 6: 56–83.
- Fehr, E. 2009. On the economics and biology of trust. *Journal of the European Economic Association* 7: 235–266.
- Fehr, E., and A. Falk. 1999. Wage rigidity in a competitive incomplete contract market. *Journal of Political Economy* 107: 106–134.
- Fehr, E., and K. M. Schmidt. 1999. A theory of fairness, competition, and cooperation. *Quarterly Journal of Economics* 114: 817–868.
- Fehr, E., and K. M. Schmidt. 2007. Adding a stick to the carrot? The interaction of bonuses and fines. *American Economic Review* 97 (2): 177–181.
- Fehr, E., G. Kirchsteiger, and A. Riedl. 1993. Does fairness prevent market clearing? An experimental investigation. *Quarterly Journal of Economics* 108: 437–459.
- Fehr, E., G. Kirchsteiger, and A. Riedl. 1998. Gift exchange and reciprocity in competitive experimental markets. *European Economic Review* 42: 1–34.
- Fehr, E., L. Goette, and C. Zehnder. 2009. A behavioral account of the labor market: The role of fairness concerns. *Annual Review of Economics* 1: 355–384.
- Fehr, E., S. Gächter, and G. Kirchsteiger. 1997. Reciprocity as a contract enforcement device: Experimental evidence. *Econometrica* 65 (4): 833–860.
- Fischbacher, U., C. M. Fong, and E. Fehr. 2009. Fairness, errors and the power of competition. *Journal of Economic Behavior and Organization* 72: 527–545.
- Goolsby, K. A., and K. L. Unmuth. 2008. Demand for teachers sparks hiring war in Dallas-Fort Worth. *The Dallas Morning News* (May 12): A1.
- Guerra, G., and D. J. Zizzo. 2004. Trust responsiveness and beliefs. *Journal of Economic Behavior and Organization* 55: 25–30.
- Hannan, R. L. 2005. The combined effects of wages and firm profit on employee effort. *The Accounting Review* 80: 167–188.
- Hannan, R. L., R. Krishnan, and A. H. Newman. 2008. The effects of disseminating relative performance in tournaments and individual performance compensation plans. *The Accounting Review* 83: 893–913.
- Hansen, F. 2005. Currents in compensation and benefits. *Compensation and Benefits Review* 37: 6–17.
- Hecht, G., I. Tafov, and K. L. Towry. 2012. Performance spillover in a multi-task environment. *Contemporary Accounting Research* 20 (2): 563–589.
- Heider, F. 1958. *The Psychology of Interpersonal Relations*. New York, NY: Wiley.
- Hu, L., and P. M. Bentler. 1999. Cutoff criteria for fit indexes in covariance structure analysis: Conventional criteria versus new alternatives. *Structural Equation Modeling* 6: 1–55.
- Kelley, H. H. 1973. The processes of causal attribution. *American Psychologist* 28: 107–128.
- Kelley, H. H., and J. L. Michela. 1980. Attribution theory and research. *Annual Review of Psychology* 31: 457–501.
- Kramer, R. M. 1999. Trust and distrust in organizations: Emerging perspectives, enduring questions. *Annual Review of Psychology* 50: 569–598.
- Kuang, X., and D. V. Moser. 2009. Reciprocity and the effectiveness of optimal agency contracts. *The Accounting Review* 84: 1671–1694.
- Kuang, X., and D. V. Moser. 2011. Wage negotiation, employee effort, and firm profit under output-based versus fixed-wage incentive contracts. *Contemporary Accounting Research* 28 (2): 616–642.
- Mayer, R. C., J. H. Davis, and F. D. Schoorman. 1995. An integrative model of organizational trust. *The Academy of Management Review* 20: 709–734.
- McCabe, K. A., M. L. Rigdon, and V. L. Smith. 2003. Positive reciprocity and intentions in trust games. *Journal of Economic Behavior & Organization* 52: 267–275.
- Medland, M. E. 2003. When to pay signing bonuses. *HR Magazine* 48 (12): 99–102.
- Miller, J. W. 2008. Offshore rig workers call the shots on jobs. *Wall Street Journal* (November 10): B1.

- National Association of Colleges and Employers (NACE). 2008. *Job Outlook 2009*. Bethlehem, PA: NACE.
- Poe, A. C. 1999. Signing bonuses: A sign of the times. *HR Magazine* (September): 104–112.
- Rabin, M. 1993. Incorporating fairness into game theory and economics. *American Economic Review* 83: 1281–1302.
- Rigdon, M. 2009. Trust and reciprocity in incentive contracting. *Journal of Economic Behavior & Organization* 70: 93–105.
- Rosenfeld, D., R. Folger, and H. F. Adelman. 1980. When rewards reflect competence: A qualification of the overjustification effect. *Journal of Personality and Social Psychology* 39: 368–376.
- Rosnow, R. L., and R. Rosenthal. 1995. “Some things you learn aren’t so”: Cohen’s paradox, Asch’s paradigm, and the interpretation of interaction. *Psychological Science* 6: 3–9.
- Roth, A. E., V. Prasnikar, M. Okuno-Fujiwara, and S. Zamir. 1991. Bargaining and market behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An experimental study. *American Economic Review* 81: 1068–1095.
- Rousseau, D. M., S. B. Sitkin, R. S. Burt, and C. Camerer. 1998. Not so different after all: A cross-discipline view of trust. *Academy of Management Review* 23: 393–404.
- Salamon, S. D., and S. L. Robinson. 2008. Trust that binds: The impact of collective felt trust on organizational performance. *Journal of Applied Psychology* 93: 593–601.
- Sansone, C. 1986. A question of competence: The effects of competence and task feedback on intrinsic interest. *Journal of Personality and Social Psychology* 51: 918–931.
- Sen, A. 1997. Maximization and the act of choice. *Econometrica* 65: 745–779.
- Slonim, R., and E. Garbarino. 2008. Increases in trust and altruism from partner selection: Experimental evidence. *Experimental Economics* 11: 134–153.
- Sobel, J. 2002. Can we trust social capital? *Journal of Economic Literature* 40: 139–154.
- Sobel, J. 2005. Interdependent preferences and reciprocity. *Journal of Economic Literature* 43: 392–436.
- Sprinkle, G. B. 2000. The effects of incentive contracts on learning and performance. *The Accounting Review* 75: 299–326.
- Tafkov, I. 2013. Private and public relative performance information under different compensation contracts. *The Accounting Review* 88 (1): 327–350.
- Tversky, A., and D. Kahneman. 1991. Loss aversion in riskless choice: A reference dependent model. *The Quarterly Journal of Economics* 106: 1039–1062.
- Uchitelle, L. 1998. Signing bonus now a fixture farther down the job ladder. *The New York Times* (June 10): D2.
- Van Wesep, E. D. 2010. Pay (be)fore performance: The signing bonus as an incentive device. *The Review of Financial Studies* 23 (10): 3812–3848.
- WorldatWork. 2000. *Sign-on Bonus Survey*. Scottsdale, AZ: WorldatWork.
- WorldatWork. 2005. *Bonus Program Practices*. Scottsdale, AZ: WorldatWork.
- WorldatWork. 2007. *Cash Bonus Plans and Recognition Programs*. In *The WorldatWork Handbook of Compensation, Benefits & Total Rewards: A Comprehensive Guide for HR Professionals*, 338–364. Scottsdale, AZ: WorldatWork.
- WorldatWork. 2009. *Bonus Program Practices*. Scottsdale, AZ: WorldatWork.
- Zizzo, D. J. 2010. Experimenter demand effects in economic experiments. *Experimental Economics* 13: 75–98.

Debt Analysts' Views of Debt-Equity Conflicts of Interest

Gus De Franco

University of Toronto

Florin P. Vasvari

London Business School

Dushyantkumar Vyas

University of Toronto

Regina Wittenberg-Moerman

The University of Chicago

ABSTRACT: We investigate how the tone of sell-side debt analysts' discussions about debt-equity conflict events affects the informativeness of debt analysts' reports in debt markets. Conflict events such as mergers and acquisitions, debt issuance, share repurchases, or dividend payments potentially generate asset substitution or wealth expropriation by equity holders. We document that debt analysts routinely discuss these conflict events in their reports. More importantly, discussions about conflict events that we code as negative are associated with increases in credit spreads and bond trading volume. Consistent with the informational value of debt analysts' discussions in secondary debt markets, we find that negatively coded conflict discussions predict higher bond offering yields in the primary bond market. In additional analyses, we measure the tone of debt analysts' discussions based on their disagreement with the

We appreciate the helpful comments of the Beverly R. Walther (editor), John Harry Evans III (senior editor), two anonymous referees, Ray Ball, Phil Berger, Gavin Cassar (discussant), Ole-Kristian Hope, Stephannie Larocque, Scott Liao, Mingzhi Liu (discussant), Peter Morris, Managing Director of the European credit research team at Morgan Stanley, Richard Phelan, Head of European High Yield Research Team at Deutsche Bank, K. Ramesh, Liang Tan (discussant), participants at AAA 2010 Annual Meeting, CAAA 2010 Annual Meeting, FARS 2011 Midyear Meeting, and seminar participants at the Humboldt Graduate School of Business, London Business School, Massachusetts Institute of Technology, Rice University, Stanford University, The University of Chicago, University of California, Los Angeles, University of Florida, University of Mannheim Business School, University of Notre Dame, and University of Toronto. We thank the following research assistants, who helped us to collect, code, and process the debt analysts' text data: Karen Chang, Konstantin Chestopalov, Hae-Me Chung, Richard Gao, Wendy Gu, Jackson Huang, Jingtian Huang, Tanya Kauchhur, Angel Liang, Karan Malhotra, Sunyoung Park, Paul Xing, Sherry Xu, Kosta Zabashta, David Zhang. We gratefully acknowledge the financial support of The Initiative on Global Markets at The University of Chicago Booth School of Business, Social Sciences and Humanities Research Council of Canada, Rotman School of Management, the University of Toronto, and London Business School RAMD Fund.

Editor's note: Accepted by Beverly R. Walther.

Submitted: September 2010

Accepted: September 2013

Published Online: October 2013

tone of equity analysts' discussions and find that the informativeness of debt analysts' reports is higher when our coding indicates that conflict events are viewed negatively by debt analysts but positively by equity analysts.

Keywords: *debt analysts; equity analysts; debt-equity conflict events; bond volume; bond yield; CDS spreads.*

JEL Classifications: *G12; G14; G32; M49.*

I. INTRODUCTION

Recent studies show that investors react to the publication of debt analysts' reports and the recommendations they provide, suggesting that debt analysts act as important information intermediaries in debt markets (Johnston, Markov, and Ramnath 2009; De Franco, Vasvari, and Wittenberg-Moerman 2009; Gurun, Johnston, and Markov 2011). Aside from providing a review of firms' financial performance in their reports, debt analysts discuss and analyze the effects of debt-equity conflict events that potentially generate asset substitution and wealth expropriation by equity holders, such as mergers and acquisitions, debt issuance, share repurchases, and dividend payments (hereafter, "conflict events"). These discussions are likely to provide debt investors with important information and can explain debt markets' reactions to debt analysts' reports. We assess the tone of debt analysts' discussions of conflict events using textual analysis and investigate the effects of the tone on the borrowing firms' credit spreads, bond trading, and cost of debt.

While previous studies establish the existence of events that lead to debt-equity conflicts, as discussed in Section II, these studies find mixed results with respect to the economic importance of such conflicts to debt holders. If these conflict events meaningfully affect debt holders' wealth, then debt analysts' discussions should be informative to debt investors. However, if conflict discussions do not provide material information or are not timely, then these discussions will not expand the information set available to debt holders.

Our sample consists of conflict-event discussions extracted from a unique hand-collected dataset of 11,025 reports issued by sell-side debt analysts (hereafter, "debt analysts") on U.S. firms. Specifically, from each debt analyst's report we extract the text around a comprehensive list of keywords associated with events such as mergers and acquisitions, divestments, dividend payments, stock repurchases, or debt issuance. We then manually code a randomly selected set of text extractions and use it to calibrate a Naive Bayesian model that estimates whether the analyst's discussion is negative, neutral, or positive. In the last step, we use this model to produce a predicted tone for the full sample of text extractions used in our tests.

We document that debt analysts routinely discuss events that are likely to generate debt-equity conflicts in their reports. These discussions not only follow, but also anticipate firm announcements of these conflict events, highlighting their informativeness. Further, based on our coding, debt analysts typically interpret conflict events as non-negative, with the majority of events being interpreted as mixed or neutral news for debt investors. This finding suggests that conflict events are not always expected to reduce debt holders' wealth. Instead, they can provide a positive signal about the firm's performance or news that is either less negative than debt holders initially expected or simply not material.

Turning to our debt market tests, we first find that debt analysts' reports are more relevant to debt investors, as evidenced by stronger debt market reactions, for reports coded as containing negative discussions of debt-equity conflicts. We document that firms' credit default swap (CDS) spreads increase when debt analysts' discussions about conflict events take on a negative tone. Relative to reports coded as containing positive or neutral discussions, reports that include

negatively coded discussions increase the abnormal CDS spread by a statistically significant 12 basis points over the five-day window centered on the date of a debt analyst's report. In particular, we show that greater gains to equity holders, as evidenced by positive abnormal equity returns, are associated with greater losses to debt holders around the publication of debt analysts' reports with negatively coded discussions of conflict events. This result suggests that debt analysts' negative conflict discussions reflect potential wealth transfers from debt holders to equity holders. To corroborate these CDS reactions, we show that when the tone of debt analysts' discussions about conflict events is coded as negative, relative to reports coded as having a neutral or positive tone, the daily bond trading volume is higher by a statistically significant 1.15 percent of the bonds' principal over the five-day window report date. The bond trading volume reaction further highlights the importance of the tone of analysts' discussions to debt market investors.

Consistent with the reactions in secondary debt markets, we document that the negatively coded conflict discussions in debt reports that precede the issuance of bond securities in the primary market predict the initial pricing of these bonds. Their yield to maturity increases with the negative tone of the conflict discussions in reports published in the six-month period prior to the bond issuance. Relative to discussions coded as neutral or positive, discussions of conflicts coded negatively increase the offering yield by a statistically significant 27 basis points. This finding suggests that debt analysts' views also provide additional information to primary bond market investors and affect firms' cost of debt financing.

Next, we test whether the impact of discussions coded as having a negative tone about conflict events varies cross-sectionally with firms' credit ratings across our market variables—daily CDS spread changes, daily bond trading volume, and yields on new bond issues. We find some evidence suggesting that negatively coded conflict discussions have stronger associations with our market variables for lower-rated debt, consistent with an increase in the informational value of debt analysts' discussions in settings where debt investors demand more information about downside risk.

To provide additional evidence that debt analysts' conflict-event discussions capture the construct of conflict between debt and equity holders, we develop and test an alternative tone measure based on the disagreement in the coded tone of conflict discussions between debt and equity analyst reports. This measure captures situations where these discussions are coded negative for debt analysts but positive for equity analysts, reflecting the idea that the news is negative for debt holders but positive for equity holders. In both the primary and secondary market tests, our main findings and inferences continue to hold using this alternative measure.

Our use of a "Naïve Bayes" computational process to estimate the tone of the text in debt analysts' reports follows studies such as Antweiler and Frank (2004) and Li (2010). The method uses the training dataset that we manually coded to calculate the probabilities that the tone of the text is positive, neutral, and negative, respectively, for each word in the text. The Naive Bayes model then takes the words in the text and aggregates these probabilities by word to form a categorical prediction (positive, neutral, or negative) about the text. This computational process allows us to dramatically increase our research sample compared to a manual coding of the text.

We acknowledge that our Naïve Bayes computational process introduces measurement error, which we can identify by comparing the tone of text extractions based on manual coding with the model's predictions. As we discuss in Section III, the classification performance metrics suggest our procedure performs reasonably well. Further, while we do not expect the measurement error in our variable of interest—reports with a negatively coded tone—to be systematically related to any of our debt market reaction measures, we nonetheless perform a series of additional tests to examine the sensitivity of our findings to the classification accuracy of the Naive Bayes computational process. More specifically, we restrict the analyses to manually coded extractions, focus on reports with multiple negatively coded text extractions, run all the tests excluding neutrally coded

discussions, and examine the relation between the tone of the conflict discussions and analysts' investment recommendations. While we believe that the results of these tests mitigate concerns regarding the measurement error in our main variable of interest, we acknowledge that measurement error limits our ability to make strong inferences about the effect of the tone of conflict discussions on debt markets' reactions to debt analysts' reports.

Our study contributes to the literature across several dimensions. First, we contribute to research on the role of information intermediaries such as analysts, rating agencies, and the press in shaping firms' information environments.¹ We provide unique evidence showing that the tone of debt analysts' discussions reflects new information generated by analysts that expands the information set available to debt market investors. Our conflict-event discussion variable exhibits an incremental effect after controlling for firm-initiated disclosures and the disclosures of other intermediaries, such as rating agencies and equity analysts.

Second, we add to the small but growing literature on debt analysts. Although many studies investigate equity analysts, there is limited evidence on the role of debt analysts. This is notable given that the U.S. corporate debt market is the largest and most important source of capital for U.S. firms (Bessembinder and Maxwell 2008). Our analyses complement the recent debt analyst studies of Johnston et al. (2009), De Franco et al. (2009), and Gurun et al. (2011) by providing insight into the type of information produced by debt analysts and the debt market's reaction to their reports. We also present new evidence on the relation between debt analysts' reports and firms' borrowing costs in the primary bond market.²

Third, in contrast with prior literature, we provide and examine an empirical measure that captures debt holders' concerns about wealth expropriation and asset substitution without the need to directly rely on bond returns around conflict events. Bond returns might not capture these concerns due to infrequent bond trading, divergent reactions due to different bond features, or reactions to firm fundamentals. Short-window bond returns around the actual event also do not capture events anticipated by the market. In addition, prior research on debt-equity conflicts, with the exception of Parrino and Weisbach (1999), has focused on only one type of conflict event in isolation, without assessing the interactions between conflict events and their simultaneous effect on debt holders' wealth. This further contributes to the inconclusive evidence that prior research provides with respect to the importance of conflict events to debt holders. Our approach of coding the tone of debt analysts' discussions about debt-equity conflicts, while affected by measurement error, allows us to document the importance of these conflict events as well as to determine whether and how debt holders' wealth is affected.

The next section reviews the literature and develops our research question. Section III describes our data and the measurement of debt analysts' discussions of conflict events. Section IV discusses the results of our tests. Section V concludes.

¹ See, for example, studies by Frankel, Kothari, and Weber (2006), Ederington and Goh (1998), Dichev and Piotroski (2001), Beaver, Shakespeare, and Soliman (2006), Fang and Peress (2009), and Bushee, Core, Guay, and Hamm (2010).

² To our knowledge, no work has been done that investigates the impact of debt analysts' reports on firms' bond yields or their cost of debt. Even prior work on the relation between equity analysts' disclosures and the cost of debt is limited. Güntay and Hackbarth (2010) and Mansi, Maxwell, and Miller (2011) find that equity analysts' forecast dispersion and forecast revision volatility increase bond yield spreads.

II. LITERATURE REVIEW AND RESEARCH QUESTIONS

Background on Debt Analysts³

Most corporate bonds are traded in an opaque over-the-counter dealer market and often become absorbed in “buy-and-hold” portfolios shortly after issuance. The resulting low transparency of bond trading provides debt analysts with an opportunity to play a significant role in supporting the informational efficiency of the corporate debt market. Similar to equity analysts, debt analysts evaluate firms’ fundamental performance and firm-specific events, but with a strong emphasis on how a firm’s performance and events impact its credit quality. The investment recommendations they provide are driven by the perceived effect of a firm’s performance and actions on its ability to pay off its debt, given market factors and the specific contractual features of debt agreements that shield debt investors from potential losses. Debt analysts’ perceived market mispricing of a bond security, relative to other similar bonds, contributes to the issuance of “buy” or “sell” recommendations. Typically, debt analysts’ investment recommendations (e.g., buy, hold, or sell) have an approximate 90-day horizon and cover all bond securities issued by a given firm that are actively trading in the secondary market. De Franco et al. (2009) find that the distribution of debt analysts’ recommendations is skewed positively, but less positively than the distribution of equity analysts’ recommendations. This result suggests that debt analysts act less strategically and provide more negative information than equity analysts, which likely meets the asymmetric demand for negative information by bond investors.

Aside from investment recommendations, debt analysts’ reports also provide discussions of firms’ financial performance, including a detailed examination of EBITDA, free cash flows, capital expenditures, liquidity, and leverage ratios, as well as discussions of lending market conditions or relative bond pricing. More importantly, the reports provide extensive discussions of risks associated with events that potentially give rise to conflicts of interest between bondholders and shareholders, such as mergers and acquisitions, new debt issuances, asset sales, dividend payments, or share repurchases. The value of debt analysts’ reports should, *inter alia*, stem from the analyses they provide about conflict events. Recommendations without proper analysis and support are more likely to be ignored by institutional clients, who represent the vast majority of bond investors (Warga 2004). Further, debt analysts are compensated in part based on institutional client surveys that assess the value of debt analysts’ research.

Importance of Debt Analysts’ Discussions of Conflict Events

We broadly partition the type of news that debt analysts discuss into news concerning firms’ fundamentals and news about the distribution of wealth between debt and equity investors. Our study focuses on the latter type of news concerning conflict events. These conflict events occur when firms take unexpected investment and financing actions that could increase the value of shareholder equity claims, but that decrease the value of the outstanding debt (Jensen and Meckling 1976; Myers 1977). Debt analysts’ views on a particular conflict event are likely to take into account the level of protection that debt holders receive from the protective features embedded in the debt contract, including covenants, collateral, put provisions, and sinking fund provisions. Hence, if analysts were to issue a negative view of a conflict event, then it may be a sign that they perceive these protective features to be insufficient.

³ Part of this background information is based on our discussions with debt analysts and other debt market participants. In particular, we thank Richard Phelan, Managing Director of the European High Yield Research Team at Deutsche Bank, and Peter Morris, Managing Director of the European credit research team at Morgan Stanley.

On the one hand, because debt-equity conflicts have the potential to negatively affect debt holders' wealth, debt holders will demand information about the downside risk that these events pose on their claims, and debt analysts are expected to meet this demand. If the potential conflict is material, regardless of whether it has been anticipated, debt analysts are likely to monitor, analyze, and discuss its effect on the value of outstanding debt securities in their reports. Based on these arguments, our central prediction is that the negative tone of debt analysts' conflict discussions is relevant to debt investors and generates debt market reactions.

On the other hand, the extent and economic importance of such conflict events to debt holders' wealth, as documented by extant studies, is far from clear. In the case of spin-offs, for example, Hite and Owers (1983) and Schipper and Smith (1983) find no evidence of a wealth transfer at the time of the announcement, while Maxwell and Rao (2003) document a wealth transfer from debt holders to equity holders. With respect to cash payouts, Woolridge (1983) and Handjinicolaou and Kalay (1984) do not find that dividend payments have a detrimental effect on bondholders. In contrast, Dhillon and Johnson (1994) and Maxwell and Stephens (2003) do find evidence supporting a wealth transfer from debt holders. Finally, while Warga and Welch (1993) and Billet, King, and Mauer (2004) document a loss in debt holders' wealth associated with leveraged buyouts and acquisitions, Marais, Schipper, and Smith (1989) and Lehn and Poulsen (1991) find that leveraged buyouts do not result in a wealth transfer. If conflict events have a minimal negative effect on debt holders' wealth, as some of these studies suggest, the debt market should not react to the negative tone of debt analysts' conflict discussions. Similarly, the debt market should not react to the tone of conflict discussions if they do not provide relevant new information or are not timely.

We expect the importance of negative conflict discussions to vary cross-sectionally with firms' debt ratings. For higher quality debt, debt prices are less sensitive to new information, because either the likelihood of a loss is very small or the upside potential is already limited. In contrast, the price of lower rated debt is highly sensitive to negative information (De Franco et al. 2009; Easton, Monahan, and Vasvari 2009), making debt analysts' negative conflict discussions more valuable to debt investors. Hence, we predict that the negative tone of debt analysts' conflict discussions leads to greater market reactions for lower rated debt. However, it is possible that debt analysts' discussions of conflict events already take into account the riskiness of the bond security. Hence, when the tone of these discussions is conditional on the bond's credit rating, it may not generate differential market reactions. Thus, the impact of negative conflict-event discussions across rating categories is ultimately an empirical question.

Empirically, we measure debt market reactions by changes in secondary market CDS spreads⁴ and bond trading volume⁵ and by the level of the offering yields on bond issues in the primary market. We predict that CDS spreads, bond volume, and bond yields are positively related to the negative tone of debt analysts' conflict discussions.

⁴ CDS contracts capture the credit quality level in terms of the probability of default and have the advantage of trading far more frequently than the underlying bond securities (Hull, Predescu, and White 2004). The low frequency of bond trade data makes it difficult to conduct short-window tests in bond markets (Goodhart and O'Hara 1997; Hotchkiss and Ronen 2002). CDS spreads are also much less affected by liquidity problems, changes in interest rates, and contract design features that are specific to bond securities (Bushman, Le, and Vasvari 2010). These features, such as option-like provisions and collateral requirements, can be difficult to control for in empirical tests.

⁵ Volume reactions are associated with new information (Kim and Verrecchia 1991), as well as with information that leads to a divergence of opinion among investors (Harris and Raviv 1993; Kim and Verrecchia 1994). If debt analysts' reports matter to debt investors, then they should generate additional trading.

III. DATA AND MEASUREMENT OF DEBT ANALYSTS' CONFLICT-EVENT DISCUSSIONS

Collection of Debt Analysts' Reports

We obtain debt analysts' reports from First Call Thomson ONE Analytics. For each report we manually code the analyst name, brokerage firm, report date, investment recommendation, and the name of the company covered by the report. The majority of debt analysts' recommendations are at the firm level, i.e., they refer to all of the firm's bonds that are actively trading in the secondary market.⁶ If the broker has not already done so, we standardize the debt analysts' recommendations into three categories: buy, hold, or sell.⁷ We also collect current Moody's or Standard & Poor's rating information from the reports.

We select reports about U.S. corporate firms with bond securities for the years 2002 to 2007. The start of this period aligns with the availability of bond trading data from the Trade Reporting and Compliance Engine (TRACE) database. We exclude reports on Real Estate Investment Trusts, on financial institutions such as banks or insurance companies, reports about companies domiciled in non-U.S. countries, reports about macro-economic variables, industry reports, and reports that are aggregated by time, where this last type often simply repeats previously published information in reports (Stickel 1995; De Franco and Hope 2011). Reports missing an investment recommendation are excluded from the analysis. This procedure for collecting debt analysts' reports is similar to that used in De Franco et al. (2009).

Measuring the Tone of Debt Analysts' Discussions of Conflict Events

We employ a computational-linguistic method to code the text in debt analysts' reports. Our objective is to obtain a detailed quantitative understanding of debt analysts' discussions of events that signal potential deterioration in the issuer's credit quality from the perspective of debt holders.⁸ We take the following five steps.

Step 1

We convert the debt analysts' reports from a PDF format to text files and identify certain generic conflict-event "keywords." These words and phrases indicate an event that can indicate a wealth transfer from debt to equity holders. Classified into five main categories, the italicized keywords (and all their linguistic derivatives) are: (1) Mergers & Acquisitions (M&A)—*Merger, Acquisition, Leveraged Buyout, and Management Buyout*; (2) Wealth distribution—*Stock Repurchase, Dividend, and Cash Distribution*; (3) Divestments—*Spin-off and Asset Sales*; (4) Financial risk—*Debt and Leverage*; and (5) Other—*Capital Expenditure (or Capex), Event Risk, Shareholder Pressure, Shareholder Friendly, Shareholder Focus, Wealth Transfer, and Covenants*. The financial risk category is meant to capture a variety of management actions that could lead to changes in financial risk and, hence, to a wealth transfer from equity to debt holders (e.g., debt

⁶ Debt analysts occasionally provide recommendations at the bond level. For our sample, in almost all cases when recommendations at the bond level are available for each of the firm's bonds, these recommendations are identical.

⁷ While most brokers typically employ these three recommendation levels, some use a system of five levels (Strong Buy, Buy, Hold, Sell, Strong Sell). The standardization to three levels allows us to pool the recommendations together in our empirical tests.

⁸ Our Naive Bayesian updating algorithm approach, described below, builds on burgeoning research that uses the tools of computational linguistics (Tetlock 2007; Tetlock, Saar-Tsechansky, and Macskassy 2008; Kothari et al. 2009; Li 2010; Rogers, Van Buskirk, and Zechman 2011). Beyer, Cohen, Lys, and Walther (2010) call for more work in this area.

issuances or restructurings). The last category captures miscellaneous discussions about capital expenditures, changes in covenants or the likelihood of covenant violations, and general notions such as “event risk” and management’s “equity focus.”

Step 2

For each keyword in a debt analyst’s report, we extract the words contained in the sentence that contains the keyword and the two sentences before and after it. For each report, we then accumulate the text extractions by keyword. For example, if the word “repurchase” appears more than once in the report, we extract the surrounding text around each “repurchase” keyword and group these sentences together. Thus, for each keyword, we are able to extract from the report all of the analyst’s discussion about that word. Each keyword text extraction corresponds to a different conflict event.

Step 3

We use a “Naive Bayes” approach (implemented via the “Rainbow” program, publicly available from Andrew McCallum at <http://www.cs.cmu.edu/~mccallum/bow> [McCallum 1996]) to classify the keyword text extractions identified in the previous step as positive, negative, or neutral (see Antweiler and Frank [2004] and Li [2010] for examples of this approach). To estimate the Naive Bayes model, we need a set of data to “train” the algorithm. We create this training dataset by manually classifying 5,933 randomly selected text extractions as positive, neutral, or negative.⁹ Appendix B provides examples of text extractions and the manual coding for each extraction. We then use the text and the manually coded tone from the training dataset to generate a Naive Bayes model that predicts the analyst’s tone for all the text extractions in our sample—negative, positive, or neutral—conditional on the words in the text extraction.

Step 4

In line with our focus on the negative tone of debt analysts’ conflict discussions when we estimate our tests, we classify the extractions as negative or non-negative, which includes positive or neutral. To determine the accuracy of the Naive Bayes algorithm, in Appendix C we calculate the within-sample accuracies of the classifications by comparing the algorithmic classifications with the manual classifications. We compute several measures that capture our model’s performance, based on the prior literature both in accounting and finance (Barber and Lyon 1996; Chava and Jarrow 2004; Stein 2005; Kothari, Leone, and Wasley 2005; Dechow, Ge, Larson, and Sloan 2011) and medical statistics (Glas, Lijmer, Prins, Bonsel, and Bossuyt 2003; Lasko, Bhagwat, Zou, and Ohno-Machado 2005; Fawcett 2006). Appendix C provides the definition of each of these measures.

The *Sensitivity* is 65 percent, reflecting a Type I error of 35 percent. This error represents the probability of the model incorrectly predicting a non-negative tone when the tone is actually negative. The *Specificity* is 87 percent, reflecting the Type II error of 13 percent. The Type II error represents the probability of the model incorrectly predicting a negative tone when the tone is actually non-negative. We also estimate an aggregate measure, the *Accuracy* of our model, which reflects both false positive and false negative errors and captures the probability that the tone is

⁹ Ten research assistants (RA) carried out this task in a standardized manner with the guidance of a process document we developed. Given the subjective nature of the task, two different research assistants independently coded each text extraction. We then engaged a third RA to reconcile any differences. Untabulated analyses indicate that the accuracy of our model using this coding approach is similar to an alternative approach in which we limit the sample to those text extractions for which the first two RAs coding the extraction agreed on the classification.

correctly classified. *Accuracy* is equal to 85 percent for our model. A less common approach to look at these error rates is to assess the percentage of true negative (non-negative) tone discussions when the model predicts a negative (non-negative) tone. *Positive Predictive Value* is 33 percent and *Negative Predictive Value* is 96 percent. As a benchmark, our model's performance is not quite as good as that of the Antweiler and Frank (2004) algorithm. While Antweiler and Frank (2004) do not provide any formal assessment of their measurement error, the in-sample classifications they report allow us to calculate the five performance measures. Their model's *Accuracy* (89 percent), *Sensitivity* (72 percent), *Specificity* (95 percent), and *Negative Predictive Value* (91 percent) are marginally better than or in line with those that we report, but their *Positive Predictive Value* (83 percent) is much better than the 33 percent value that we report.¹⁰

In untabulated analysis, we also estimate the out-of-sample prediction ability of our algorithm, by randomly partitioning the manually coded training dataset into two parts. We use one part to train and estimate the model and the other to test the classification accuracy. Our out-of-sample performance metrics are overall lower with *Sensitivity* of 47 percent, *Specificity* of 87 percent, *Accuracy* of 83 percent, *Positive Predictive Value* of 26 percent, and *Negative Predictive Value* of 94 percent. In comparison, Li's (2010) Naive Bayes model has an out-of-sample *Accuracy* of between 67 percent and 90 percent, but he does not report other performance measures. To address the potential impact of the classification accuracy of a Naive Bayes algorithm on our findings and inferences, we report the results of several sensitivity analyses in Section IV.

Step 5

As our tests are at the report level, we create a measure per report by aggregating the extraction-level scores. To each extraction, we assign the value of -1 if the Naive Bayes algorithm classifies the text extraction as negative, 0 if it is classified as neutral, and 1 if it is classified as positive. Then, the score for each extraction is weighted by the number of words in that extraction scaled by the total number of words across all extractions in the report. We then sum the weighted scores of all extractions in a report, where by construction the measure lies between -1 and 1 . Last, we define *Conflict Discuss Neg* as an indicator variable that takes a value of 1 if the continuous report-level score is negative, and 0 otherwise.¹¹

Bond and CDS Data

We retrieve secondary market corporate bond trades from the National Association of Securities Dealers' (NASD) TRACE system and the Mergent Fixed Income Securities Database (FISD). Because TRACE's coverage of firms is not complete until February 2005, we augment our TRACE bond data with that of FISD when necessary. We match the debt analysts' report data with the combined TRACE-FISD data at the firm level by manually merging the debt analysts' reports using the issuer's firm name. We successfully match approximately 82 percent of our debt analysts' report data. We then match the borrowers with debt analysts' reports and bond trading information with Compustat. We limit the sample to firms with publicly traded equity, because our analysis requires equity analyst coverage. The final step involves merging this extensive dataset with the textual analysis data. We obtain CDS data from the database provided by Markit Partners. To maintain contract homogeneity, we focus on the most liquid CDS contract, which is the five-year

¹⁰ To facilitate comparisons with our Negative versus Non-Negative contingency table, we convert Antweiler and Frank's (2004) classifications of Buy, Hold, and Sell into two categories, Buy and Non-Buy.

¹¹ We find that our main inferences are similar in two untabulated robustness tests. First, we employ the continuous measure of conflict discussions instead of the indicator variable *Conflict Discuss Neg*. Second, as an alternative measure of debt analysts' conflict discussions, we use the General Inquirer classification algorithm.

TABLE 1
Summary of Sample Selection Process

Total debt analyst reports (from 2002 to 2007).	22,247
Reports that could be converted into text files.	13,525
Reports that have at least one conflict-event text extraction classified by Rainbow.	11,052
Corresponding number of conflict-event text extractions contained in these reports (one text extraction per keyword per report).	39,121
Mean (median) number of conflict-event text extractions per debt analyst's report.	3.54 (3.00)

CDS contract with the modified restructuring clause. Finally, we obtain bond yield to maturity at issuance and other bond features from Mergent FISD.

Test Sample

Of a potential sample of 22,247 reports, we successfully “parse” 13,525 reports from PDF to text format, of which 11,052 have at least one conflict-event extraction classified by Rainbow. This final set of reports generates 39,121 conflict-event extractions. Each report has a mean (median) of 3.5 (3.0) text extractions, which corresponds to a report mentioning a mean of 3.5 (median 3.0) unique conflict-event keywords. Table 1 summarizes this selection process. Our regression tests have smaller samples due to additional restrictions on data availability to calculate the variables in each specification.

IV. RESULTS

Content Analysis of Debt Analysts’ Conflict-Event Discussions

The frequency of conflict events is presented in Panel A of Table 2. Debt analysts extensively discuss M&A activities, with 63 percent of the reports including such a discussion. In 32 percent of the reports, analysts discuss wealth distribution, including stock repurchases and dividend payments. Divestment discussions characterize 18 percent of the reports. The financial risk category is the most frequently discussed event; it appears in 91 percent of the reports.

Panel B of Table 2 reports the classification of conflict events by the tone of analysts’ discussions according to the Naive Bayes model. The vast majority of conflict-event extractions are classified as non-negative, with neutral discussions being the most frequent. This result holds for all conflict-event categories. A neutral tone is consistent with analysts monitoring conflict events and reporting to debt investors that conflict events are proceeding as expected by debt market participants (see Appendix B). Debt analysts use a positive tone in approximately 27 percent, 20 percent, and 29 percent of the discussion extractions related to M&A, wealth distribution, and divestments, respectively, even though these events are typically perceived as negative news for bond holders. While we cannot rule out the possibility of error in our coding, this positive tone can be driven by the conflict event having a smaller than expected negative effect or its acting as a signal of the operating fundamentals’ strength. For example, in some cases, analysts discuss management’s decision to continue its share repurchase program, but at a reduced repurchase rate. As another example, repurchases are often associated with better-than-expected operating results, providing a net positive wealth effect for debt holders.

TABLE 2
Analysis by Conflict Event

Panel A: Frequency Distribution of Conflict Events

Conflict-Event Keyword	Number of Conflict-Event Text Extractions	% of Total (39,121) Conflict-Event Text Extractions	% of Total (11,052) Reports
M&A			
M&A	6,822	17.4	61.7
LBO	232	0.6	2.1
MBO	50	0.1	0.5
	7,104	18.1	63.3
Wealth distribution			
Repurchase	2,857	7.3	25.8
Dividend	2,187	5.6	19.7
	5,044	12.9	31.8
Divestment			
Asset sale	1,852	4.7	16.7
Spinoff	165	0.4	1.5
	2,017	5.1	17.6
Financial risk			
Leverage	7,722	19.7	69.9
Debt	9,788	25.0	88.5
Deleverage	711	1.8	6.4
	18,221	46.5	91.3
Other			
Covenant	1,646	4.2	14.9
Capex	4,464	11.4	40.4
Equity focus	352	0.9	3.2
Event risk	273	0.7	2.5
	6,735	17.2	45.8
Total	39,121	100.0	

Panel B: Naive Bayes Classification by Conflict-Event Category

Conflict-Event Keyword	Negative (%)	Non-Negative (%)		
		Total (%)	Neutral (%)	Positive (%)
M&A	17.2	82.8	56.1	26.7
Wealth distribution	24.0	76.0	55.7	20.3
Divestment	18.3	81.7	52.6	29.1
Financial Risk	15.2	84.8	59.9	24.9
Others	14.0	86.0	65.9	20.1
Total	16.6	83.4	59.3	24.1

This table provides a content analysis of conflict-event discussions in debt analysts' reports with a conflict-event discussion. The frequency of conflict events is presented in Panel A. Panel B reports the classification of conflict events by the tone of the analyst's discussions according to the Naive Bayes model. In the last column of Panel A, the sum of each keyword percentage is not equal to the total category percentage in bold at the report level, since each report can have multiple keywords from the same category.

TABLE 3

Timing of Other Events Relative to Debt Analysts' Conflict Discussion

Announcement	Day 0 = Date of Debt Analyst's Report with a Conflict Discussion		
	Before (Day -30 to -3)	During (Day -2 to +2)	After (Day +3 to +30)
M&A	18.19%	1.52%	17.32%
Repurchase	1.07%	0.21%	1.37%
Dividend	15.60%	1.42%	16.48%
Rating Change	5.78%	0.72%	6.23%
Watchlist	6.37%	0.79%	6.42%

This table presents the frequency of actual conflict events around the publication date of debt analysts' reports with conflict discussions. We retrieve the announcement dates of these events from Thompson One (formerly SDC Platinum) and Mergent. For example, the first row shows that for 18.19 percent of these reports with conflict discussions, firms announced M&A-related activity in the period ranging from 30 to 3 trading days before the report. Approximately 1.52 percent of conflict-discussion reports are contemporaneous (i.e., day -2 to +2) with a firm's M&A announcement. For 17.32 percent of these debt analyst reports with conflict discussions, firms announced M&A-related activity in the period from 3 to 30 days after the report.

Timing of Other Events Relative to Debt Analysts' Conflict Discussions

To better understand the informational value of debt analysts' reports, we study the timing of debt analysts' reports relative to the timing of actual conflict events. Debt analysts decide when to issue reports that contain discussions of conflict events and could issue reports concurrent with, after, or in anticipation of a conflict event. Table 3 presents the frequency of actual conflict events around the publication date of debt analysts' reports with conflict discussions. We retrieve the announcement dates of these events from Thompson ONE. For example, the first row shows that, for 18.2 percent of the reports with conflict discussions, firms announced M&A-related activity in the period ranging from 30 to 3 trading days before the report. Approximately 1.5 percent of conflict-discussion reports are contemporaneous (i.e., day -2 to +2) with a firm's M&A announcement. For 17.3 percent of these debt analyst reports, firms announced M&A-related activity in the period from 3 to 30 days after the report. Note that these percentages do not add up to 100 percent because a particular conflict event, such as M&A-related activity in this example, may not occur around a report. The remaining rows of the table demonstrate that this pattern for M&A announcements is similar to other conflict events, suggesting that debt analysts discuss both past and future potential events. The evidence that debt analysts anticipate conflict events provides some support for the notion that the tone of their discussion may provide relevant and timely information to debt investors.

In additional untabulated analysis, we examine the timing of the negatively coded conflict-event discussion in debt analysts' reports relative to credit rating downgrades and additions to a negative watchlist. We test for the relative timing of these events by performing Granger causality analyses. We estimate multivariate logistic models that predict rating downgrades and negative watchlist additions using lagged values of debt analysts' negatively coded discussions, negative rating agency actions, and CDS spread changes as independent variables (see Beaver et al. [2006] and De Franco et al. [2009] for similar Granger causality analyses). We find that debt analysts' discussions coded as negative significantly lead rating downgrades and negative watchlist additions up to two months prior to the month of the respective credit event. These untabulated tests also show that negatively coded conflict discussions lead increases in CDS spreads. These results are

consistent with the idea that debt analysts' discussions anticipate the deterioration in credit quality that is associated with conflicts, which suggests that these discussions should be informative to debt market investors.

Effect of Debt Analysts' Conflict Discussions on Secondary Debt Market Reactions Daily CDS Spread Changes

Table 4 presents the empirical analysis of the effect of the negatively coded tone of conflict discussions on CDS spreads. This analysis includes observations for *all* trading days during the six-year sample period for which we have CDS spread data available for the sample firms. Within this framework, we have the flexibility to control for firm, analyst, rating agency, and macro-economic disclosures in the same test. This approach allows us to estimate the true relations between market reactions and information events and to compare the magnitude of the CDS spread changes across different events. For each event, including the publication of a debt report, we assign the news to the announcement day and to each of the two trading days immediately before and after it.¹² We estimate:

$$\Delta \text{CDS Spread}_{it} = \beta_0 + \beta_1 \text{Conflict Discuss Neg}_{it} + \beta_2 \text{Debt Report Controls}_{it} + \beta_3 \text{Other Information Controls}_{it} + \text{Rating Effects} + \eta_{it}. \quad (1)$$

$\Delta \text{CDS Spread}$ is the daily change in the CDS spread for firm i less the change in the average CDS spread of all entities that have the same credit rating category on a given trading day. Increases in CDS spreads indicate deterioration in an issuer's credit quality. We expect the *Conflict Discuss Neg* coefficient to be positive, which indicates that negatively coded conflict discussions increase CDS spreads. We control for the levels and changes in debt analysts' recommendations. The model also includes equity analysts' recommendations, which allow us to better analyze whether debt analysts provide different information than equity analysts. Conflict discussions represent an important means of distinguishing the analysis of debt analysts' from that of equity analysts. If the contents of the debt analysts' reports were limited only to an analysis of a firm's fundamentals, such as the level of EBITDA or earnings, then the distinction between equity and debt analysts' reports would be minimal because both types of analysts would monitor and evaluate the firm's performance in similar ways. We control for rating agencies disclosures because rating agencies are established as important information providers in the bond market. Finally, we control for firms' abnormal equity returns and negative earnings announcements to proxy for operating fundamentals of the business that are common to debt and equity investors.

The results of estimating Equation (1) are presented in column (1) of Table 4. The coefficient on *Conflict Discuss Neg* is positive and marginally significantly different from 0, consistent with debt analysts' negatively coded conflict discussions predicting a deterioration in an issuer's creditworthiness beyond the standard investment recommendation. Relative to reports with positive or neutral discussions, reports that include negatively coded discussions increase the CDS spread by 12 basis points [2.4 basis points $\times$ 5 days] over the five-day window centered on the date of the debt analysts' reports. As expected, equity returns are negatively associated with CDS spread changes.

In the following specifications in Table 4, to better capture the idea of wealth transfers from debt to equity holders driven by conflict events, we focus on situations in which the news is

¹² We use a five-day window because it is difficult to precisely identify when the information in the debt analysts' reports is released to debt investors. Consistent with this claim, De Franco et al. (2009) show that the market reaction to a debt analyst's report is spread over the five days centered on the day of the report. Other examples of debt-market and analyst-related papers that use five-day windows in their analysis include Hand, Holthausen, and Leftwich (1992), Asquith et al. (2005), and Bonner, Hugon, and Walther (2007).

TABLE 4

The Effect of the Tone of Debt Analysts' Conflict Discussions on Daily CDS Spread Changes

	Pred.	Full Sample (1)	Sample Restricted to Positive News for Equity Investors	
			(2)	(3)
<i>Conflict Discuss Neg</i>	+	0.024* [1.89]	-0.029* [-1.75]	-0.022 [-1.58]
<i>Conflict Discuss Neg × Equity Return</i>	+		2.880** [2.25]	
<i>Conflict Discuss Neg: Above A Rating × Equity Return</i>	+			1.663*** [3.40]
<i>Conflict Discuss Neg: BBB-A Rating × Equity Return</i>	+			1.887*** [3.25]
<i>Conflict Discuss Neg: Below BBB Rating × Equity Return</i>	+			3.215** [2.46]
<i>Debt Buy Recommendation</i>	-	-0.004 [-0.74]	-0.002 [-0.21]	-0.002 [-0.24]
<i>Debt Sell Recommendation</i>	+	0.001 [0.21]	-0.003 [-0.22]	-0.001 [-0.06]
<i>Debt Recommendation Upgrade</i>	-	-0.008 [-0.94]	-0.026** [-2.40]	-0.023** [-2.20]
<i>Debt Recommendation Downgrade</i>	+	0.017 [1.40]	0.027** [2.34]	0.025** [2.19]
<i>Equity Buy Recommendation</i>	?	-0.001 [-0.64]	0.003 [1.32]	0.002 [0.83]
<i>Equity Sell Recommendation</i>	?	-0.005 [-0.61]	-0.005 [-0.35]	-0.004 [-0.31]
<i>Rating Agency Action Neg</i>	+	0.035 [1.09]	0.017 [0.84]	0.017 [0.81]
<i>Equity Return</i>	-	-1.302*** [-6.57]	-1.532*** [-4.24]	-1.466*** [-4.63]
<i>Earnings Neg</i>	+	0.016 [1.04]	0.009 [0.44]	0.013 [0.81]
Rating Effects		Yes	Yes	No
No. of Obs.		364,595	178,213	178,213
Adjusted R ² (%)		1.27	1.34	0.79

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using two-tailed tests. Coefficient t-statistics are in brackets.

This table presents an analysis of daily CDS spread change reactions to the coded tone of debt analysts' conflict discussions, controlling for other events that could affect CDS spread changes. The dependent variable is $\Delta CDS\ Spread$. In column (1), the full sample includes observations for all trading days for which we have CDS spread data available during the six-year period studied for each of the sample firms. For the remaining columns, we limit the sample to observations that represent positive news for equity holders, as proxied by positive abnormal stock returns. We estimate the OLS model as a panel and cluster the standard errors at the firm level. Variables are defined in Appendix A.

negative for debt holders and positive for equity holders. In particular, we restrict the sample to observations in which news for equity holders is positive, as proxied by daily abnormal stock returns greater than 0. We focus on this sample because prior literature shows that wealth transfer from debt holders to equity holders is associated with excess gains to equity holders (Warga and Welch 1993; Maxwell and Rao 2003; Maxwell and Stephens 2003). By excluding observations with non-positive abnormal stock returns, we are more confident that our results are not driven by situations in which equity holders' wealth is unaffected or has declined.

We also interact the *Conflict Discuss Neg* and *Equity Return* variables. Because of the interaction, in this specification the coefficient on *Equity Return* will capture days when debt analysts' conflict discussions are positive or neutral or when there are no debt analysts' reports. The expected coefficient is negative. The coefficient on the interaction term captures situations when equity holders' wealth improves while debt analysts provide negative discussions of conflict events, which directly corresponds to debt-equity conflict events. Hence, we predict a positive coefficient on the *Conflict Discuss Neg* $\times$ *Equity Return* interaction variable, as greater gains to equity holders should be associated with greater losses to debt holders around conflict events. This specification is consistent with Alexander, Edwards, and Ferri (2000), who show that on days with no conflict events (with conflict events), positive equity returns are associated with an increase (decrease) in bond prices. Note that the relative frequency of report to non-report days and the ratio of conflict discussions coded as negative versus non-negative remain generally the same after we restrict the sample to positive daily abnormal stock returns.

Column (2) of Table 4 shows the results of this specification. The coefficient on *Equity Return* is negative and statistically significant, consistent with both equity returns and CDS spread changes capturing news about operating fundamentals. It is also consistent with the evidence in Collin-Dufresne, Goldstein, and Martin (2001) and Hotchkiss and Ronen (2002), who document modest comovement between bond and equity market returns. As expected, the coefficient on the *Conflict Discuss Neg* $\times$ *Equity Return* interaction variable is positive and statistically significant, which is consistent with a wealth transfer from debt to equity holders when equity holders' wealth improves and debt analysts provide negative discussions of conflict events. In terms of control variables, CDS spread changes are increasing (decreasing) with debt analysts' recommendation downgrades (upgrades). An untabulated F-statistic indicates that the rating fixed effects are jointly statistically significant.¹³

We next examine how this result changes as a function of the creditworthiness of the firm. We define three new conflict discussion indicator variables based on the credit rating category of the firm. The first variable, *Conflict Discuss Neg: Above A Rating*, equals 1 if debts analysts' conflict discussion is coded negatively and the firm's senior debt rating is above A, 0 otherwise. Similarly, *Conflict Discuss Neg: BBB-A Rating* (*Conflict Discuss Neg: Below BBB Rating*) equals 1 if debt analysts' conflict discussion is coded negative and the firm's senior debt rating ranges from BBB to A (is below BBB), 0 otherwise. We then interact these three variables with *Equity Return*.¹⁴ The

¹³ In untabulated analyses, we conduct the following alternative CDS-reaction test. We examine whether the analysts' view of conflict events explains changes in CDS spreads around debt analysts' reports using a sample of reports that contain a discussion of conflict events. We regress the change in the CDS spread over the event window around debt analysts' reports on *Conflict Discuss Neg*. We control for the change in the market CDS spread, characteristics of debt and equity analysts' recommendations, and whether a bond is on a negative watchlist. We find a significant increase in the CDS spread when debt analysts' reports include negatively coded discussions of conflict events, particularly when equity returns over the same event window are positive. These results provide further support for our inferences.

¹⁴ Because we are interested in examining the effect of negatively coded conflict discussions as a function of the creditworthiness of a firm, we omit rating fixed effects from this specification. The same holds for the volume and offering yield analyses when we conduct similarly motivated cross-sectional tests.

results are presented in column (3) of Table 4. As expected, we find that the coefficients increase as creditworthiness declines. An untabulated test indicates that the difference in coefficients between the highest and lowest credit risk categories is marginally statistically significant ($p\text{-value} = 0.10$), but the difference between the medium and low-risk categories is not significant ($p\text{-value} = 0.15$). These findings provide some support to our cross-sectional prediction that the news provided by negatively coded debt analysts' conflict discussions is more valuable for lower rated debt.

Daily Bond Trading Volume

As in the empirical analyses of CDS spread changes, we use panel data observations for all trading days during our six-year period for each of the sample firms and test whether bond trading reacts to the negative tone of debt analysts' discussions of conflict events. For each event, we assign the news to a five-trading-day event window and estimate the following model:

$$\begin{aligned} \text{Bond Volume}_{it} = & \beta_0 + \beta_1 \text{Conflict Discuss Neg}_{it} + \beta_2 \text{Debt Report Controls}_{it} \\ & + \beta_3 \text{Other Information Controls}_{it} + \text{Rating Effects} + \text{Year Effects} + \eta_{it}. \end{aligned} \quad (2)$$

Bond Volume is the dollar volume of principal traded on a given day, scaled by the size of the bond on the issue date, averaged over all the firm's bonds.¹⁵ We control for the existence of a debt analyst's report, and whether the report contains a conflict discussion, so that we can focus our interpretation on the effect of negative versus positive or neutral discussions. We also control for conflict discussions that are longer, as evidenced by a larger word count. More extensive discussions should generate stronger volume reactions since they are likely to provide more information or discuss the effects of a conflict event in more detail.¹⁶

While the motivation for the remaining control variables in this test remains the same as in the CDS spread analyses, the set of control variables for this test is larger and somewhat different. Because CDS spread changes reflect either improving or deteriorating news, the control variables for that test consist of signed control variables, such as recommendation upgrades or downgrades. In addition to these signed control variables, because trading volume is an unsigned measure, the set of bond trading volume controls includes indicator variables for the existence of events. These other controls include such conditions as actual firm conflict-event announcements (e.g., M&A, dividend, or stock repurchase announcement), an equity analyst's report, a rating agency action, an earnings announcement, and a federal funds rate change announcement. In general, we expect the coefficient on each of the event indicator variables to be positive, consistent with incremental bond volume occurring around the news associated with each event. Note that our tests allow for the differential effect of negative versus positive news by including separate indicator variables for negative news, when appropriate. Consistent with the demand for negative news (Easton et al. 2009; De Franco et

¹⁵ In untabulated analysis, we conduct the volume-reaction tests using an alternative *Bond Volume* measure. We define this measure as the aggregate total trading volume for all the firm's traded bonds, scaled by the sum of the face value of all the firm's outstanding bonds. Our findings are similar both statistically and economically.

¹⁶ In untabulated analysis, we conduct the following alternative volume-reaction test. We examine whether analysts' views of conflict events affect abnormal bond trading volume around debt analysts' reports that contain a discussion of conflict events. We regress abnormal volume, a firm's daily volume around the report less the daily volume on non-event days, on *Conflict Discuss Neg*. We control for characteristics of debt and equity analysts' recommendations, equity volume, text count, the time to maturity of the bond, a bond's trading frequency, and whether a bond is on a negative watchlist. We find a greater abnormal volume around debt analysts' reports that include negatively coded conflict discussions. These results provide further support for the notion that investors' reaction to debt analysts' reports is more pronounced when debt analysts view conflict events negatively.

al. 2009), we expect indicator variables that designate negative news to have an incremental effect on trading volume. Last, we add equity volume to control for broad firm-level information.

The results in column (1) in Table 5 show a positive and significant coefficient on *Conflict Discuss Neg* of 0.230 (t-statistic = 6.05). Over the five-day event window, an incremental 1.15 percent ($5 \text{ days} \times 0.230 \text{ percent per trading day}$) of the bonds' principal is traded around debt analysts' reports with negative conflict discussions, relative to debt analysts' reports with positive or neutral conflict discussions. This volume "reaction" is similar to the incremental response to negative announcements by rating agencies, as captured by the coefficient on *Rating Agency Action Neg*, an important source of information in debt markets.

To better assess the role of negative versus positive conflict discussions, in column (2), we include a separate indicator for positive conflict discussions (*Conflict Discuss Pos*), which is defined in a way that is analogous to our main negative conflict variable. The coefficient on *Conflict Discuss Pos* is not significant, suggesting that the discussions of conflict events that are coded positively do not produce a greater volume reaction than those that are neutral. Overall, these results are consistent with our prediction that debt investors find the reports of debt analysts more informative if they contain negatively coded discussions of conflict events.

In terms of control variables, positive coefficients on *Debt Report* and *Debt Report with Conflict Discussion* reflect the incremental bond volume around the publication of debt analysts' reports in general and reports with a non-negative conflict discussion. Longer reports are negatively correlated with trading volume, possibly because shorter reports allow debt analysts to provide more timely information. Debt analysts' reports with sell recommendations generate higher trading volume than reports with hold recommendations, while those with buy recommendations generate about the same volume as those with hold recommendations. Debt recommendation upgrades lead to greater volume reactions than do downgrades. Equity trading volume is positively related to debt trading volume, consistent with some news affecting both equity and debt holders. Bond market reactions are also greater for debt analysts' recommendations than for equity analysts' recommendations, consistent with De Franco et al. (2009). As expected, credit rating actions, particularly negative actions, generate additional volume. Earnings news generates trading volume reaction; the reaction is greater to negative earnings news than to positive or neutral news. With respect to macroeconomic news, higher volume is associated with a Federal Funds rate change.

In column (3), we find some support for our prediction that the market reaction to conflict discussions is increasing in credit risk. The coefficients on *Conflict Discuss Neg: BBB-A Rating* and *Conflict Discuss Neg: Below BBB Rating* are positive and statistically significant. Although there is no difference between the *Conflict Discuss Neg* coefficients for the *Below BBB* and *Above A* ratings, the untabulated p-value for the test of the difference between the *BBB-A Rating* and *Above A Rating* categories is 0.04. The greatest volume reaction in the middle-risk group suggests that these bonds have higher liquidity.

In sum, the results for CDS spread changes and bond trading volume support the idea that negatively coded debt analysts' discussions about conflict events lead to debt market reactions that are incremental to their recommendations. These results are consistent with debt analysts' conflict-event discussions being important to debt investors. Our inferences compare with Asquith, Mikhail, and Au (2005), who find that the manually coded tone of the text in equity analysts' reports is related to the equity market reaction. The evidence contrasts somewhat with Kothari, Li, and Short (2009), who find that the tone of the text in equity reports has an insignificant impact on the cost of capital, concluding that disclosures by equity analysts appear to be heavily discounted by equity markets.

TABLE 5

The Effect of the Tone of Debt Analysts' Conflict Discussions on Daily Bond Trading Volume

	Pred.	(1)	(2)	(3)
<i>Conflict Discuss Neg</i>	+	0.230*** [6.05]	0.235*** [5.79]	
<i>Conflict Discuss Pos</i>	+		0.012 [0.38]	
<i>Conflict Discuss Neg: Above A Rating</i>	+			0.059 [0.33]
<i>Conflict Discuss Neg: BBB-A Rating</i>	+			0.399*** [6.74]
<i>Conflict Discuss Neg: Below BBB Rating</i>	+			0.106** [2.13]
<i>Debt Report</i>	+	0.138*** [9.73]	0.138*** [9.74]	0.123*** [8.07]
<i>Debt Report With Conflict Discussion</i>	+	0.125*** [5.68]	0.121*** [5.15]	0.122*** [5.44]
<i>Text Count High</i>	+	-0.063** [-2.17]	-0.065** [-2.21]	-0.077*** [-2.61]
<i>Debt Buy Recommendation</i>	+	0.014 [0.62]	0.014 [0.62]	-0.009 [-0.37]
<i>Debt Sell Recommendation</i>	+	0.108*** [3.86]	0.108*** [3.86]	0.137*** [4.72]
<i>Debt Recommendation Upgrade</i>	+	0.147*** [4.46]	0.147*** [4.45]	0.143*** [4.27]
<i>Debt Recommendation Downgrade</i>	+	0.060* [1.87]	0.060* [1.87]	0.052 [1.55]
<i>Equity Volume</i>	+	0.002*** [2.94]	0.002*** [2.94]	0.002*** [2.61]
<i>Conflict Event Firm Announcement</i>	+	0.002 [0.25]	0.002 [0.25]	0.039*** [5.66]
<i>Equity Report</i>	+	0.066*** [6.56]	0.066*** [6.56]	0.078*** [7.23]
<i>Equity Buy Recommendation</i>	+	-0.035*** [-4.14]	-0.035*** [-4.14]	-0.032*** [-3.70]
<i>Equity Sell Recommendation</i>	+	0.034** [2.21]	0.034** [2.21]	0.021 [1.35]
<i>Rating Agency Action</i>	+	0.109*** [6.33]	0.109*** [6.33]	0.088*** [4.82]
<i>Rating Agency Action Neg</i>	+	0.242*** [8.18]	0.242*** [8.18]	0.254*** [8.43]
<i>Earnings</i>	+	0.016** [2.21]	0.016** [2.21]	0.011 [1.52]
<i>Earnings Neg</i>	+	0.096*** [5.27]	0.096*** [5.27]	0.055** [2.41]
<i>Fed Change</i>	+	0.022*** [3.21]	0.022*** [3.21]	0.021*** [3.09]
Rating Effects		Yes	Yes	No
Year Effects		Yes	Yes	Yes
No. of Obs.		952,502	952,502	952,502
Adjusted R ² (%)		2.4	2.4	0.9

(continued on next page)

TABLE 5 (continued)

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using two-tailed tests. Coefficient t-statistics are in brackets. This table presents an analysis of bond daily trading volume reactions to the coded tone of debt analysts' conflict discussions, controlling for other events that could affect bond trading volume. The dependent variable is *Bond Volume*. The underlying sample includes observations for all trading days during the 2002–2007 period studied for each of the sample firms. We estimate the OLS model as a panel and cluster the standard errors at the firm level. Variables are defined in Appendix A.

Effect of Debt Analysts' Conflict Discussions on Bond Offering Yields

To examine whether debt analysts' negative conflict discussions are also informative with respect to firms' cost of debt, we test how the tone of these discussions affects the offering yield to maturity of new bond issues. We manually match our debt analyst data with the Mergent FISD database to obtain a sample of bonds issued in the 2002 to 2008 period. We focus on reports issued within the six months prior to the bond issue. To select a more homogenous group of bonds, we exclude corporate bonds that are convertible, privately placed, issued in foreign currencies, or that have variable coupon payments. Our final sample consists of 1,013 bonds issued by 290 firms. We estimate the following OLS model at the bond-issue level:

$$\begin{aligned} \text{Bond Offering Yield} = & \beta_0 + \beta_1 \text{Conflict Discuss Neg Prior } 6M_{it} \\ & + \beta_2 \text{Debt Recommendation Prior } 6M_{it} \\ & + \beta_3 \text{Bond Characteristic Controls}_{it} \\ & + \beta_4 \text{Firm Characteristics Controls}_{it} + \beta_5 \text{Other Information Controls}_{it} \\ & + \text{Rating Effects} + \text{Industry Effects} + \eta_{it}. \end{aligned} \tag{3}$$

The dependent variable measures the cost of a new bond issue. An untabulated analysis indicates that the median bond yield for our sample is 658 basis points, with a standard deviation of 440 basis points. Our main variable of interest is *Conflict Discuss Neg Prior 6M*, which is defined at the bond-issue level, where the *6M* indicates that we calculate the average tone score across all debt analysts' reports issued over the six-month period prior to a bond's issuance. *Conflict Discuss Neg Prior 6M* takes the value of 1 if the average value of the tone score is less than 0.

We control for debt analysts' recommendations, recommendation changes, and other bond characteristics, including the number of covenants, maturity, and the offering amount. As the agency theory of covenants suggests that there is a trade-off between the number of covenant restrictions and the interest rate (Smith and Warner 1979), we expect the number of covenants to be negatively related to the offering yield. Bonds with longer maturities are usually exposed to greater interest and credit risks, and therefore are likely priced at higher yields. We also include offering amount and bond complexity in our specification, but it is not possible to predict the sign of their relations to offering yield because for each variable, arguments can be made to support both a positive and negative relation. In addition, we control for firm characteristics that include size, leverage, and interest coverage. Consistent with prior literature, we expect bond offering yields to increase with firm leverage and decrease with firm size and interest coverage. We predict a negative sign on the indicator variable for private bank debt because we expect bank monitoring to benefit bond holders (Datta, Iskandar-Datta, and Patel 1999). We include a variable that indicates whether conflict events were announced by the firm in the six-month period prior to a bond's issuance. We also add a control for equity analysts' recommendations and include the Federal Funds rate at the date of the bond's issuance. Finally, we include rating fixed effects and industry fixed effects that capture industry-level characteristics, such as asset tangibility.

The estimation of Equation (3) is presented in column (1) of Table 6. The coefficient on *Conflict Discuss Neg Prior 6M* is positive and significant, suggesting that negatively coded conflict-event discussions in debt analysts' reports preceding a bond's issuance lead to a higher bond offering yield. Economically, a report with conflict-discussion tones coded as negative increases the offering yield by 27 basis points relative to non-negative coded discussions, which represents 4.0 percent of the median bond yield of the sample firms. This evidence suggests that conflict-event discussions are associated with real effects on a company's cost of debt.

In column (2) of Table 6, we examine whether the effect of conflict discussions on offering yields depends on the bond's credit riskiness. We find some evidence to support our prediction that when debt is of low credit quality, debt yields are more sensitive to new information. The coefficients on the conflict discuss measure increase as creditworthiness declines, with the coefficient on *Conflict Discuss Neg Prior 6M: BBB-A Rating* marginally significant at the 10 percent one-sided level and the coefficient on *Conflict Discuss Neg Prior 6M: Below BBB Rating* significant at the 5 percent level. An untabulated test shows a significant difference in coefficients between the medium (highest) credit risk category and the lowest one, with a p-value of 0.08 (0.02). These results are consistent with our findings in the CDS market test, in which the credit spreads of low credit quality firms react more strongly to debt analysts' conflict discussions.

With respect to control variables, we find that the offering yield increases with leverage and decreases when the bond is complex or the firm is subject to bank monitoring. We also find that the offering yield increases when the Federal Fund rate increases. In the second column, we find that the offering yield is decreasing in bond maturity, amount, and a firm's interest coverage. Contrary to our prediction, we find that the number of covenants is positively related to the interest rate, likely due to endogeneity between the interest rate and covenants. We address this issue in column (3) by estimating a two-stage least squares (2SLS) specification, where the number of covenants is predicted in the first stage (untabulated) and added as an explanatory variable for the offering yield in the second stage. We use the number of covenants by calendar year indicators as the instrument. The strictness of covenant packages significantly deteriorated during the years of the credit boom that preceded the financial crisis (Leverage World 2006; Fitch Ratings 2007; Moody's Investor Services 2007).¹⁷ After controlling for endogeneity, our results show a significant and negative relation between the offering yield and the number of covenants as expected. Consistent with the agency theory of covenants, our results in column (3) show that the greater the number of covenants, the smaller the offering yield. More importantly, the results confirm that allowing for endogeneity between the offering yield and the number of covenants does not affect our main conclusion that negatively coded conflict discussions predict higher offering yields.¹⁸

¹⁷ In untabulated analysis, we find that the average number of covenants significantly decreased for bonds issued in 2006 relative to bonds issued in previous years, and there was a further deterioration for bonds issued in 2007 (the untabulated partial F-statistic is 3.82, with a p-value of 0.002). As we control for the Federal Funds rate, the offering yield is not directly related to year fixed effects.

¹⁸ While we address the endogeneity between bond yields and covenants, our analysis treats the rest of the bond contractual terms as exogenous. The regressions using debt contractual terms involve a variety of simultaneity and endogeneity problems, which makes it extremely difficult to find appropriate instruments (Bradley and Roberts 2004; Costello and Wittenberg-Moerman 2011). Further, for practical purposes, it is infeasible to concurrently endogenize all bond contractual terms incorporated into the yield analysis. As an untabulated robustness test, to address the joint determination of bond terms, we estimate the offering yield, the number of covenants, maturity, and the offering amount as a system of equations using a seemingly unrelated regression (SUR) model. SUR allows error terms in all four regressions to be correlated. Our findings and inferences are robust to the SUR estimation.

TABLE 6
The Effect of the Tone of Debt Analysts' Conflict Discussions on the Offering Yields of New Bond Issues

	Pred.	(1)	(2)	(3)
<i>Conflict Discuss Neg Prior 6M</i>	+	0.270** [2.02]		0.340** [2.56]
<i>Conflict Discuss Neg Prior 6M: Above A Rating</i>	+		−0.741 [−1.17]	
<i>Conflict Discuss Neg Prior 6M: BBB-A Rating</i>	+		0.940 [1.55]	
<i>Conflict Discuss Neg Prior 6M: Below BBB Rating</i>	+		0.636** [2.36]	
<i>Debt Recommendation Prior 6M</i>	−	0.050 [0.50]	0.117 [0.89]	0.052 [0.52]
<i>Debt Recommendation Change Prior 6M</i>	−	−0.201 [−0.61]	−0.155 [−0.38]	−0.180 [−0.55]
<i>Number of Covenants</i>	−	0.084** [1.98]	0.349*** [8.75]	
<i>Number of Covenants (Predicted)</i>	−			−0.168*** [−4.19]
<i>Bond Maturity</i>	+	0.176 [1.13]	−0.493*** [−4.22]	0.182 [1.16]
<i>Bond Offering Amount</i>	?	0.112 [0.89]	−0.456*** [−2.69]	0.139 [1.21]
<i>Complexity High</i>	?	−0.538*** [−3.56]	−0.340** [−2.10]	−0.471*** [−3.38]
<i>Firm Size</i>	−	−0.029 [−0.38]	−0.008 [−0.08]	−0.061 [−0.90]
<i>Leverage</i>	+	1.879*** [2.92]	3.104*** [5.07]	1.724*** [2.67]
<i>Interest Coverage</i>	−	−0.008 [−0.56]	−0.043** [−2.37]	−0.006 [−0.42]
<i>Private</i>	−	−0.455* [−1.77]	−0.519* [−1.75]	−0.463* [−1.85]
<i>Conflict Event Firm Announcement Prior 6M</i>	?	0.271** [2.23]	0.270** [2.22]	0.244** [2.05]
<i>Equity Recommendation Prior 6M</i>	−	−0.210 [−1.50]	−0.426** [−2.40]	−0.265* [−1.90]
<i>Fed Rate</i>	+	0.137*** [2.60]	0.335*** [3.09]	0.130** [2.50]
Rating Effects		Yes	No	Yes
Industry Effects		Yes	Yes	Yes
No. of Obs.		1,013	1,013	1,013
Adjusted R ² (%)		75.30	65.08	75.36

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using two-tailed tests. Coefficient t-statistics are in brackets.

This table presents an analysis of the relation between the offering yield to maturity of new bond issues and the coded tone of debt analysts' conflict discussions, controlling for other factors that could affect these yields. The dependent variable is *Bond Offering Yield*. The sample includes observations of new bond issues during 2002–2008 for which we have debt analysts' reports in the preceding six-month period. We estimate the OLS model as a panel and cluster the standard errors at the firm level.

All other variables are defined in Appendix A.

Alternative Measure of Debt Analysts' Negative Conflict Discussions

In this section, we develop and test an alternative tone measure based on the difference in the tone of conflict discussions between debt and equity analyst reports, which may more directly capture the idea of conflict between debt and equity investors. We identify a sample of 88,241 equity analysts' reports that we match by firm to each debt analyst's report.¹⁹ For the CDS and volume-related tests, we match the firm's equity analysts' reports issued within the 10-trading-day window prior to the debt report; for the offering yield analysis, the window is six months prior to the debt report. As in the case of debt analysts' reports, we extract equity analysts' discussions of conflict events and measure the discussions' tone using a Naive Bayesian model. More specifically, we repeat Steps 1 to 5 described in Section III and develop two measures of the tone of equity analysts' discussions. The difference between these two measures is driven by which set of manually coded data we use to "train" the algorithm according to the "Naive Bayes" approach (Step 3). For the first measure, we use the same training dataset of 5,933 randomly selected text extractions from debt analysts' reports that we employed to estimate the tone of debt analysts' conflict discussions. For the second measure, we use a new training dataset based directly on equity analysts' conflict discussions. We create this training dataset by manually classifying as positive, negative, or neutral 5,102 randomly selected text extractions from the matched equity analysts' reports.²⁰

Following our strategy of coding debt analysts' reports, we create a report-level score by summing up the scores from all extractions in each equity analyst's report, weighted by each extraction's respective word count. For each debt analyst report, we average the tone of the report-level scores of equity analysts' discussions across all matched equity analysts' reports, which results in two aggregate measures of the tone of equity analysts' discussions, one based on the debt analysts' reports training set and the second based on the equity analysts' reports training set. Last, our two new alternative measures of the negative tone of debt analysts' conflict discussions focus on those discussions where this tone disagrees with the tone of equity analysts' conflict discussions. *Alternative Measure #1* of debt analysts' conflict discussions takes the value of 1 if debt analysts' reports contain negative conflict discussions, when the tone of equity analysts' conflict discussions is positive, based on the first equity analyst tone proxy. *Alternative Measure #2* is defined analogously based on the second equity analyst tone proxy. These two variables indicate that when the tone of debt analysts' conflict discussions is negative, the tone of equity analysts' conflict discussions is positive 45.3 percent and 67.3 of the time, respectively.

We repeat the analysis reported in Tables 4 to 6 with these two alternative measures of the tone of debt analysts' conflict discussions and report the results in Table 7. We present the coefficient and t-statistic on our main variable of interest only. All specifications are estimated with the same number of observations and the same set of other independent variables as the previously tabulated tests. Control variable coefficients are similar to those previously presented and omitted for brevity. For comparison, the first column in Table 7 repeats the main results from Tables 4 to 6. The next two columns present the results for *Alternative Measure #1* and *Alternative Measure #2*.

¹⁹ We downloaded these reports from Investext in a manner similar to that of De Franco, Hope, Vyas, and Zhou (2013). We use the Investext header information to match Investext analyst names with I/B/E/S analyst names and verify these matched analyst-name pairs using broker names. We link companies being covered by these matched analyst-name pairs in both Investext and I/B/E/S using the Tickers, Cusips, and Company Names. Next, we convert the PDF files into text files that are merged with the header information using the unique Investext report number.

²⁰ In the equity analyst report training dataset, 11.0 percent (89.0 percent) of the text extractions are coded as negative (non-negative). This is similar to the debt analyst report training dataset in which 8.8 percent (91.2 percent) of the text extractions are coded as negative (non-negative).

TABLE 7
Alternative Measure of the Tone of Debt Analysts' Conflict Discussions

Variable	Pred.	Main Measure (for comparison) (1)	Alternative Measure #1 (2)	Alternative Measure #2 (3)
From Table 4, Column (2), Dep. Var. = Δ CDS Spread				
<i>Conflict Discuss Neg</i> $\times$ <i>Equity Return</i>	+	2.880** [2.25]	4.451*** [3.00]	2.943 [1.52]
From Table 5, Column (1), Dep. Var. = <i>Bond Volume</i>				
<i>Conflict Discuss Neg</i>	+	0.230*** [6.05]	0.110* [1.91]	0.161*** [3.99]
From Table 6, Column (1), Dep. Var. = <i>Bond Offering Yield</i>				
<i>Conflict Discuss Neg Prior 6M</i>	+	0.270** [2.02]	0.472** [1.99]	0.391** [2.79]

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using two-tailed tests. This table presents results for two alternative measures of our main variable, the coded tone of debt analysts' negative conflict discussions. The alternative measures are based on the disagreement in the coded tone of conflict discussions between debt and equity analyst reports. Our alternative hybrid measure of debt analysts' conflict discussions focuses on debt analysts' reports that contain negative conflict discussions when equity analysts' conflict discussions are positive. *Alternative Measure #1* uses the training dataset from debt analysts' reports (used in our main analysis) to estimate the equity analysts' Naive Bayesian model. *Alternative Measure #2* uses a new training dataset based directly on equity analysts' conflict discussions. The first column repeats the respective main result from Tables 4 to 6 as a benchmark for comparison. The next two columns present the results using identical tests but with one of the two alternative measures: only the coefficient and t-statistic of the main variable are tabulated. All specifications are estimated with the same number of observations and the same set of other independent variables as the previously tabulated tests, but the results for these other variables are not tabulated. All variables are defined in Appendix A.

Using *Alternative Measure #1*, we continue to find that when abnormal daily stock returns are positive, the coefficient on *Conflict Discuss Neg* $\times$ *Equity Return* is positive and statistically significant, which is consistent with a wealth transfer from debt to equity holders when analysts provide discussions of conflict events that we code as negative. The results are marginally significant at the $p < 0.10$ level, one-sided test when we use *Alternative Measure #2*. For the bond trading volume analysis, using the alternative conflict discussion measures, the coefficients on *Conflict Discuss Neg* are generally similar in both statistical and economic significance to those reported in Table 5. These results support our prediction that debt investors find the reports of debt analysts to be more informative if they contain negative discussions of conflict events. Finally, when debt analysts view conflict events negatively while equity analysts are more positive about them, the offering bond yields are significantly higher. These findings confirm our conjecture that debt analysts' discussions of conflict events are also informative with respect to the initial pricing of bond securities in the primary market.

Sensitivity Analysis: Classification Errors of the Naive Bayes Algorithm

As we discuss in Section III, the use of a Naive Bayes model introduces measurement error that may affect our findings and inferences. While descriptive statistics suggest that the misclassification

error is not systematically related to any of our debt market reaction measures,²¹ we investigate the robustness of our results to the classification accuracy of the Naive Bayes algorithm. The first set of tests we perform addresses the concern that our results could be driven by neutral and positive discussions that the algorithm misclassifies as negative. We re-run our main tests using three alternative measures of the tone of conflict discussions.

First, we re-code the *Conflict Discuss Neg* variable based on our manually coded text extractions, which allows a much more accurate classification of the debt analysts' negative conflict discussions. In this specification, the *Conflict Discuss Neg* variable takes the value 1 if the aggregate report-level score of conflict discussions based on manually collected extraction-level scores is less than 0, 0 otherwise. This approach, however, poses the problem of reduced statistical power due to the substantially smaller sample size of reports with a manually coded text extraction. In addition, because we performed manual coding at the extraction level, not the report level, some of these reports contain text extractions that have not been manually coded and are thus not accounted for when the aggregate report-level score is determined. Nevertheless, the results continue to hold for the CDS spread and volume analyses, albeit at the 10 percent significance level for the latter. We do not find a statistically significant relation between the tone of analysts' discussions and the bond offering yield; this is potentially attributed to the relatively small sample generated by the limited number of manually coded reports and the requirement that firms issue a new bond after these reports are published. Across most of these analyses, the economic significance of the tone measure, as captured by the magnitude of the coefficients, is similar to that of our original *Conflict Discuss Neg* variable.

Second, we focus on reports with more than one conflict-event discussion. We re-code the *Conflict Discuss Neg* indicator variable as equal to 1 only if all the extractions in a report are classified by the Naive Bayes algorithm as negative. We expect the classification error of the tone at the report level to be substantially lower in this case. When debt analysts view all conflict events negatively according to the algorithm, the negative tone of each discussion in a report serves as a concordance check for the tone of other negative discussions in the same report. We continue to find that the negatively coded tone of conflict discussions has a statistically significant effect on trading volume and offering yield analyses, with a slightly weaker effect on the CDS spreads (significant at the 10 percent level). The economic significance of this alternative tone measure is similar to that of *Conflict Discuss Neg* in our primary analyses.

Third, because a Naive Bayes algorithm tends to classify extractions as neutral when they actually have a positive or negative tone, we eliminate from all the analyses any neutrally coded reports. We continue to find a statistically and economically significant relation between the negatively coded conflict discussions and our main debt market reaction variables.

Finally, to provide construct validity of our tone classification, in untabulated analyses we assess the relation between *Conflict Discuss Neg* and debt analysts' recommendations and revisions to their recommendations—the main summary outputs of analyst reports. We estimate a Probit model at the debt-analyst report level with a debt analyst sell recommendation and a recommendation downgrade as dependent variables.²² We find that when debts analysts have negatively coded discussions about conflict events, the likelihood of issuing sell recommendations

²¹ We compare Δ CDS Spread, Bond Volume, Bond Offering Yield, and Equity Return across the reports that are correctly and incorrectly classified by the algorithm. Tests of differences in means, medians, standard deviations, and probability distributions of these variables indicate that these differences are not statistically significant across correctly and incorrectly classified observations.

²² In this estimation, we control for the number of words in the text extraction, whether the bond is highly traded, whether it has complex features, whether it is on a watchlist, equity forecast revisions, and rating/year fixed effects.

and negative revisions to recommendations increases by 9.2 percent and 3.8 percent, respectively. We also examine whether *Conflict Discuss Neg* explains the disagreement between the recommendations of debt and equity analysts, reflecting debt analysts' concern about debt holder wealth expropriation or asset substitution. We estimate a similar Probit model with the dependent variable taking the value of 1 when a debt analyst's recommendation is a sell and is in disagreement with the most recent equity analysts' recommendations issued for the same firm. We find that when debt analysts have negatively coded discussions about conflict events, they are 10.1 percent more likely to issue a sell recommendation that disagrees with equity analysts' recommendations.

To summarize, although none of our additional tests fully eliminates the measurement error in our main variable of interest, we believe that these tests mitigate concerns regarding the impact of measurement error on our inferences.

V. CONCLUSION

The role of information intermediaries such as equity analysts, rating agencies, and the press in shaping firms' information environments has long been of interest to researchers in accounting and finance. In this paper, we add to the prior literature by examining whether the tone of debt analysts' discussions of conflict events reflects new information generated by analysts, thereby expanding the information set available to debt markets.

Using a unique hand-collected dataset of debt analysts' reports on U.S. firms, we find that debt analysts routinely discuss in their reports events that are likely to generate debt-equity conflicts and that these discussions are often issued in anticipation of conflict events. More importantly, the tone of debt analysts' conflict-event discussions coded as negative generates significant debt market reactions, measured by higher CDS credit spreads, bond trading volume, and bond offering yields in the primary bond market. These findings support our prediction that debt analysts' discussions about conflict events are relevant to debt market investors and incremental to debt analysts' recommendations and to information provided by other information intermediaries, such as rating agencies and equity analysts.

By providing insights into the type of information produced by debt analysts that explains debt markets' reaction to their reports, we add to the emerging literature on debt analysts. Despite the importance of the U.S. corporate debt market, current evidence on the informational role of debt analysts is rare, highlighting the importance of our findings. We also contribute to the literature on debt-equity conflicts of interest. Our approach of coding the tone of debt analysts' conflict-event discussions allows us to document the importance of these conflicts to debt investors and also to determine whether and how debt holders' wealth is affected. An important limitation of our study is that our findings and inferences are potentially affected by measurement error introduced by the Naive Bayes model. Our additional sensitivity analyses mitigate this concern.

REFERENCES

- Alexander, G. J., A. K. Edwards, and M. G. Ferri. 2000. What does Nasdaq's high-yield bond market reveal about bondholder-stockholder conflicts? *Financial Management* (Spring): 23–39.
- Antweiler, W., and M. Frank. 2004. Is all that talk just noise? The information content of internet stock message boards. *Journal of Finance* 59 (3): 1259–1294.
- Asquith, P., M. B. Mikhail, and A. S. Au. 2005. Information content of equity analyst reports. *Journal of Financial Economics* 75: 245–282.
- Barber, B. M., and J. D. Lyon. 1996. Detecting abnormal operating performance: The empirical power and specification of test statistics. *Journal of Financial Economics* 41: 359–399.
- Beaver, W. H., C. Shakespeare, and M. T. Soliman. 2006. Differential properties in the ratings of certified versus non-certified bond-rating agencies. *Journal of Accounting and Economics* 42: 303–334.

- Bessembinder, H., and W. Maxwell. 2008. Markets transparency and the corporate bond market. *Journal of Economic Perspectives* 22 (2): 217–234.
- Beyer, A., D. A. Cohen, T. Z. Lys, and B. R. Walther. 2010. The financial reporting environment: Review of the recent literature. *Journal of Accounting and Economics* 50 (2-3): 296–343.
- Billet, M., D. King, and D. Mauer. 2004. Bondholder wealth effects in mergers and acquisitions: New evidence from the 1980s and 1990s. *Journal of Finance* 59 (1): 107–135.
- Bonner, S. E., A. Hugon, and B. R. Walther. 2007. Investor reaction to celebrity analysts: The case of earnings forecast revisions. *Journal of Accounting Research* 45 (3): 481–513.
- Bradley, M., and M. Roberts. 2004. *The Structure and Pricing of Corporate Debt Covenants*. Working paper, Duke University.
- Bushee, B., J. Core, W. Guay, and S. Hamm. 2010. The role of the business press as an information intermediary. *Journal of Accounting Research* 48 (1): 1–19.
- Bushman, R., A. Le, and F. Vasvari. 2010. *Implied Bond Liquidity*. Working paper, The University of North Carolina and London Business School.
- Chava, S., and R. A. Jarrow. 2004. Bankruptcy prediction with industry effects. *Review of Finance* 8: 537–569.
- Collin-Dufresne, P., R. S. Goldstein, and J. S. Martin. 2001. The determinants of credit spread changes. *Journal of Finance* 56: 2177–2207.
- Costello, A., and R. Wittenberg-Moerman. 2011. The impact of financial reporting quality on debt contracting: Evidence from internal control weakness reports. *Journal of Accounting Research* 49: 97–136.
- Datta, S., M. Iskandar-Datta, and A. Patel. 1999. Bank monitoring and the pricing of corporate public debt. *Journal of Financial Economics* 51: 435–49.
- Dechow, P. M., W. Ge, C. R. Larson, and R. G. Sloan. 2011. Predicting material accounting restatements. *Contemporary Accounting Research* 28: 17–82.
- De Franco, G., F. P. Vasvari, and R. Wittenberg-Moerman. 2009. The informational role of bond analysts. *Journal of Accounting Research* 47 (5): 1201–1248.
- De Franco, G., and O.-K. Hope. 2011. Do analysts' notes provide new information? *Journal of Accounting, Auditing and Finance* 26: 229–254.
- De Franco, G., O.-K. Hope, D. Vyas, and Y. Zhou. 2013. Analyst report readability. *Contemporary Accounting Research* (forthcoming).
- Dhillon, U., and H. Johnson. 1994. The effect of dividend changes on stock and bond prices. *Journal of Finance* 49: 281–289.
- Dichev, I. D., and J. Piotroski. 2001. The long-run stock returns following bond ratings changes. *Journal of Finance* 56: 173–204.
- Easton, P., S. Monahan, and F. Vasvari. 2009. Initial evidence on the role of accounting earnings in the bond market. *Journal of Accounting Research* 47: 721–766.
- Ederington, L., and J. Goh. 1998. Bond rating agencies and stock analysts: Who knows what when? *Journal of Financial and Quantitative Analysis* 33: 569–585.
- Fang, L., and J. Peress. 2009. Media coverage and the cross-section of expected returns. *Journal of Finance* LXIV (5): 2023–2052.
- Fawcett, T. 2006. An introduction to ROC analysis. *Pattern Recognition Letters* 27: 861–874.
- Fitch Ratings. 2007. *U.S. Leveraged Loan Covenant Decline Accelerating in 2007*. June 20. New York, NY: Fitch Ratings.
- Frankel, R., S. P. Kothari, and J. Weber. 2006. Determinants of the informativeness of analyst research. *Journal of Accounting and Economics* 41 (1-2): 29–54.
- Glas, A., J. G. Lijmer, M. H. Prins, G. J. Bonsel, and P. M. M. Bossuyt. 2003. The diagnostic odds ratio: A single indicator of test performance. *Journal of Clinical Epidemiology* 56: 1129–1135.
- Goodhart, C., and M. O'Hara. 1997. High frequency data in financial markets: Issues and applications. *Journal of Empirical Finance* 4: 73–114.
- Güntay, L., and D. Hackbarth. 2010. Corporate bond credit spreads and forecast dispersion. *Journal of Banking and Finance* 34: 2328–2345.

- Gurun, U., R. Johnston, and S. Markov. 2011. *Sell-Side Debt Analysts and Market Efficiency*. Working paper, The Ohio State University.
- Hand, J., R. Holthausen, and R. Leftwich. 1992. The effect of bond rating agency announcements on bond and stock prices. *Journal of Finance* 47: 733–752.
- Handjinicolaou, G., and A. Kalay. 1984. Wealth redistributions or changes in firm value: An analysis of returns to bondholders and stockholders around dividend announcements. *Journal of Financial Economics* 13: 35–63.
- Harris, M., and A. Raviv. 1993. Differences in opinion make a horse race. *Review of Financial Studies* 6: 473–494.
- Hite, G., and J. Owers. 1983. Security price reactions around corporate spin-off announcements. *Journal of Financial Economics* 12: 409–436.
- Hotchkiss, E. S., and T. Ronen. 2002. The informational efficiency of the corporate bond market: An intraday analysis. *Review of Financial Studies* 15: 1325–1354.
- Hull, J., M. Predescu, and A. White. 2004. The relationship between credit default swap spreads, bond yields, and credit rating announcements. *Journal of Banking and Finance* 28: 2789–2811.
- Jensen, M. C., and W. H. Meckling. 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics* 3: 305–360.
- Johnston, R., S. Markov, and S. Ramnath. 2009. Sell-side debt analysts. *Journal of Accounting and Economics* 47: 91–107.
- Kim, O., and R. Verrecchia. 1991. Trading volume and price reactions to public announcements. *Journal of Accounting Research* 29: 302–321.
- Kim, O., and R. Verrecchia. 1994. Market liquidity and volume around earnings announcements. *Journal of Accounting and Economics* 17: 41–67.
- Kothari, S. P., A. J. Leone, and C. E. Wasley. 2005. Performance matched discretionary accrual measures. *Journal of Accounting and Economics* 39: 163–197.
- Kothari, S. P., X. Li, and J. E. Short. 2009. The effect of disclosures by management, analysts, and business press on cost of capital, return volatility, and analyst forecasts: A study using content analysis. *The Accounting Review* 84: 1639–1670.
- Lasko, T. A., J. G. Bhagwat, K. H. Zou, and L. Ohno-Machado. 2005. The use of receiver operating characteristic curves in biomedical informatics. *Journal of Biomedical Informatics* 38: 404–415.
- Lehn, K., and A. Poulsen. 1991. Contractual resolution of bondholder-stockholder conflicts in leveraged buyouts. *Journal of Law and Economics* 34: 645–673.
- Leverage World. 2006. *A Review of Covenant Trends in 2006*. December 8. Available at: <http://www.covenantreview.com/Resources/Resources.aspx>
- Li, F. 2010. The information content of the forward-looking statements in corporate filings—A Naive Bayesian machine learning approach. *Journal of Accounting Research* 48 (5): 1049–1102.
- Mansi, S., W. Maxwell, and D. Miller. 2011. Analyst forecast characteristics and the cost of debt. *Review of Accounting Studies* 16 (1): 116–142.
- Marais, L., K. Schipper, and A. Smith. 1989. Wealth effects of going private for senior securities. *Journal of Financial Economics* 23: 155–191.
- Maxwell, W., and R. Rao. 2003. Do spin-offs expropriate wealth from bondholders? *Journal of Finance* 58 (5): 2087–2108.
- Maxwell, W., and C. Stephens. 2003. The wealth effects of repurchases on bondholders. *The Journal of Finance* 58 (2): 895–919.
- McCallum, A. 1996. Bow: A toolkit for statistical language modeling, text retrieval, classification and clustering. Available at: <http://www.cs.cmu.edu/~mccallum/bow>
- Moody's Investors Service. 2007. Expanding U.S. shareholder power increases potential credit risk to bondholders. *June*.
- Myers, S. C. 1977. Determinants of corporate borrowing. *Journal of Financial Economics* 5: 147–175.
- Parrino, R., and M. Weisbach. 1999. Measuring investment distortions arising from stockholder-bondholder conflicts. *Journal of Financial Economics* 53: 3–42.

- Rogers, J. L., A. Van Buskirk, and S. L. Zechman. 2011. Disclosure tone and shareholder litigation. *The Accounting Review* 86: 2155–2183.
- Schipper, K., and A. Smith. 1983. Effects of recontracting on shareholder wealth: The case of voluntary spin-offs. *Journal of Financial Economics* 12: 437–467.
- Smith, C., and J. Warner. 1979. On financial contracting: An analysis of bond covenants. *Journal of Financial Economics* 7: 117–161.
- Stein, R. M. 2005. The relationship between default prediction and lending profits: Integrating ROC analysis and loan pricing. *Journal of Banking and Finance* 29: 1213–1236.
- Stickel, S. E. 1995. The anatomy of the performance of buy and sell recommendations. *Financial Analysts Journal* 51: 25–39.
- Tetlock, P., M. Saar-Tsechansky, and S. Macskassy. 2008. More than words: Quantifying language to measure firm's fundamentals. *Journal of Finance* 63: 1437–1467.
- Tetlock, P. C. 2007. Giving content to investor sentiment: The role of media in the stock market. *Journal of Finance* 62: 1139–1168.
- Warga, A., and I. Welch. 1993. Bondholder losses in leveraged buyouts. *Review of Financial Studies* 6 (4): 959–982.
- Warga, A. 2004. *An Overview of Regulation of the Bond Markets*. Working paper, University of Houston.
- Woolridge, R. 1983. Dividend changes and security prices. *Journal of Finance* 38: 1607–1615.

APPENDIX A
Variable Definitions

Variable	Definition
Alternative Measure #1	This alternative measure of the tone of debt analysts' negative conflict discussions is based on the disagreement in the tone of conflict discussions between debt and equity analyst reports. We extract equity analysts' discussions of conflict events from a matched sample of equity analysts' reports and measure the tone of their discussions using the Naive Bayesian model. This alternative measure equals 1 if debt analysts' reports contain negative conflict discussions while equity analysts' conflict discussions are positive (i.e., more positive than the median of the equity analysts' conflict discussion). This alternative measure uses the training dataset of 5,933 randomly selected text extractions from debt analysts' reports (used in our main analysis) to estimate the equity analysts' Naive Bayesian model.
Alternative Measure #2	The same as <i>Alternative Measure #1</i> , except that the equity analysts' Naive Bayesian model is based on training data that consists of a randomly selected and manually coded set of equity analysts' conflict discussions.
Bond Maturity	Logarithm of the number of years left to maturity at the time of the bond's issuance.
Bond Offering Amount	Logarithm of the offering amount of a given bond issue.
Bond Offering Yield	Bond issue's offering yield at issuance.
Bond Volume	Dollar volume of principal traded on a given day, scaled by the face value of the bond on the issue date, averaged over all the firm's bonds.
ΔCDS Spread	Change in the Credit Default Swap spread during a given trading day less the change in the average Credit Default Swap spread of all entities that have the same credit rating category on a given trading day. To measure the latter we use four credit rating categories: AAA to AA−, A+ to BBB+, BBB to BB, and BB− to D.
Conflict Discuss Neg	First, at the debt-analyst-report conflict-event level (i.e., the text-extraction level), we create a score that equals −1 if the Naive Bayes algorithm (implemented via Rainbow) classifies the text as negative, 0 if the text is classified as neutral, and 1 if it is classified as positive. Each analyst report can produce multiple conflict-event text extractions, with a maximum of one extraction for each conflict-event keyword. For analysis at the analyst-report level, we create a report-level score, created by summing up the scores from all extractions associated with each report weighted by each extraction's respective word count. <i>Conflict Discuss Neg</i> is an indicator variable that takes a value of 1 if the report-level score is negative, 0 otherwise.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
<i>Conflict Discuss Neg: Above A Rating</i>	Indicator variable that takes on a value of 1 if <i>Conflict Discuss Neg</i> is 1 for a firm rated higher than “A,” 0 otherwise.
<i>Conflict Discuss Neg: BBB-A Rating</i>	Indicator variable that takes on a value of 1 if <i>Conflict Discuss Neg</i> is 1 for a firm rated between “BBB” and “A,” 0 otherwise.
<i>Conflict Discuss Neg: Below BBB Rating</i>	Indicator variable that takes on a value of 1 if <i>Conflict Discuss Neg</i> is 1 for a firm rated lower than “BBB,” 0 otherwise.
<i>Conflict Discuss Neg Prior 6M</i>	An indicator variable that takes on a value of 1 if the average value of the conflict-event tone score for debt reports issued for a firm during the 180-day window prior to the bond issuance date is less than 0, 0 otherwise.
<i>Conflict Discuss Pos</i>	An indicator variable that takes a value of 1 if the report-level conflict-event tone score is positive, 0 otherwise.
<i>Conflict Event Firm Announcement</i>	Indicator variable that equals 1 in the five-day period centered on the date of an M&A, dividend, or stock repurchase announcement by a firm, 0 otherwise. Announcement dates are from the SDC Platinum and CRSP databases.
<i>Conflict Event Firm Announcement Prior 6M</i>	Indicator variable that equals 1 if there is an M&A or stock repurchase announcement by a firm in the 180-day window prior to the bond issuance date, 0 otherwise. Announcement dates are from the SDC Platinum database.
<i>Complexity High</i>	Indicator variable that equals 1 if a firm’s complexity index is above the sample’s median, 0 otherwise. The complexity index is based on the following bond characteristics: callable, convertible, credit enhancement, putable, foreign currency, floating rate coupon, variable rate coupon, combination of floating/fixed coupon, non-standard payment frequency, non-standard accrual frequency, pay-in-kind, and sinking fund. The complexity index is estimated as the sum of the bond complexity characteristics, where each characteristic is assigned the value of 1. The index is based on the average complexity of a firm’s bonds.
<i>Debt Recommendation Prior 6M</i>	The average of debt analysts’ recommendations for debt reports issued for a firm during the 180-day window prior to the bond issuance date. The recommendation is coded as equal to 1 if the debt analyst’s recommendation is a sell, 2 if it is a hold, and 3 if it is a buy.
<i>Debt Recommendation Change Prior 6M</i>	The average of the change in debt analysts’ recommendations for debt reports issued for a firm during the 180-day window prior to the bond issuance date. The change in recommendation is coded as equal to –1 if the debt analyst’s recommendation change is a downgrade, 0 if the recommendation does not change, and 1 if the recommendation change is an upgrade.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
Debt Report	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst's report, 0 otherwise.
Debt Report with Conflict Discussion	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst report that contains a conflict discussion, 0 otherwise.
Debt Buy Recommendation	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst's buy recommendation, 0 otherwise.
Debt Sell Recommendation	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst's sell recommendation, 0 otherwise.
Debt Recommendation Upgrade	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst's recommendation upgrade, 0 otherwise.
Debt Recommendation Downgrade	Indicator variable that equals 1 in the five-day period centered on the date of a debt analyst's recommendation downgrade, 0 otherwise.
Earnings	Indicator variable that equals 1 in the five-day period centered on the date of a firm's earnings announcement, 0 otherwise. Data are from Compustat.
Earnings Neg	Indicator variable that equals 1 in the five-day period centered on the date of a firm's earnings announcement if earnings before extraordinary items are negative, 0 otherwise.
Equity Buy Recommendation	Indicator variable that equals 1 in the five-day period centered on the date of an equity analyst's buy recommendation, 0 otherwise.
Equity Recommendation Prior 6M	The average of equity analysts' recommendations for a firm during the 180-day window prior to the bond issuance date. The recommendation is coded as equal to 1 if the equity analyst's recommendation is a sell, 2 if the recommendation is a hold, and 3 if it is a buy.
Equity Report	Indicator variable that equals 1 in the five-day period centered on the date of an equity analyst's report, 0 otherwise.
Equity Return	Equity return during a given trading day less the daily return of the CRSP value-weighted index of equities.
Equity Sell Recommendation	Indicator variable that equals 1 in the five-day period centered on the date of an equity analyst's sell recommendation, 0 otherwise.
Equity Volume	Number of shares traded on a given day, scaled by the number of shares outstanding.
Fed Change	Indicator variable that equals 1 in the five-day period centered on the date of the federal funds rate changes, 0 otherwise.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
<i>Fed Rate</i>	The prevailing Federal Funds rate on the day prior to the bond issuance date.
<i>Firm Size</i>	Logarithm of total assets, measured at the end of the calendar quarter preceding the quarter in which the bond was issued.
<i>Interest Coverage</i>	Ratio of interest expense to income before interest and taxes, measured at the end of the quarter preceding the quarter in which the bond was issued.
<i>Leverage</i>	The ratio of total liabilities to total assets, measured at the end of the quarter preceding the quarter in which the bond was issued.
<i>Number of Covenants</i>	Total number of covenants that are present in the bond contract.
<i>Number of Covenants (Predicted)</i>	Predicted total number of covenants that are present in the bond contract based on the year of bond issuance as a first-stage instrument.
<i>Private</i>	Indicator variable that takes the value of 1 if, at the time of the bond's issuance, a firm has syndicated loans outstanding (as indicated by the Dealscan database), 0 otherwise.
<i>Rating Agency Action</i>	Indicator variable that equals 1 in the five-day period centered on the date of a rating agency action, including rating change, watchlist addition, or outlook change, and 0 otherwise.
<i>Rating Agency Action Neg</i>	Indicator variable that equals 1 in the five-day period centered on the date of a negative rating agency action, including a rating downgrade, negative watchlist addition, or a negative outlook change, and 0 otherwise.
<i>Text Count High</i>	Indicator variable that equals 1 if the number of words in each text extraction is above the median number of words for all debt analyst reports in the sample.

APPENDIX B

Examples of Text Extracted Around Conflict-Event Keywords from Debt Analysts' Reports

Example of a Negative Classification of a Repurchase Discussion

Text Excerpt from CIBC World Markets, June 26, 2003 on Navistar

In fact, management spoke of reversing its restricted payment covenant as early as the second quarter of 2004 to allow the company to take these actions. Essentially, once the company meets its 2.5 times interest earned ratio test, the covenant would fall away. We, obviously, do not believe that the repurchase of common equity shares would be in bondholders' best interest and, therefore, view this as an additional negative for the credit. On the positive side, it seems that Navistar is benefiting from the additional volumes provided through its Blue Diamond joint venture with Ford Motor Company, and it appears that Navistar has been gaining market share thus far in 2003. Heading into 2004, we expect the company's credit statistics to continue to improve.

Example of a Positive Classification of a Repurchase Discussion

Text Excerpt from Bear Stearns, September 25, 2005 on General Mills

GIS affirmed its intent to reduce debt by \$2 billion within that time period, but only committed to debt reduction of \$450 million during this fiscal year that leaves the bulk of the debt to be reduced closer to the back end of its time frame. Nonetheless, we were impressed by the strong debt reduction language and management's indication that stock repurchase of any magnitude will not take place until the debt reduction targets are achieved. At the end of the first quarter, debt/EBITDA was 3.96x compared to 4.57x the prior year, while interest coverage has improved to 4.2x versus 3.49x a year ago. Maintain our marketweight.

Example of a Neutral Classification of an M&A Discussion

Text Excerpt from Bear Stearns, February 24, 2003 on Mediacom

Management continues to view its business plan as being more than adequately funded, and it expects to become free cash flow positive in mid-2003, as its plant rebuild nears completion. Debt maturities continue to remain fairly moderate, aggregating only about \$50 million through 2005. No significant acquisitions are expected over the near-term; management actually may see a better payoff in potentially repurchasing company stock (although its current focus appears to be on generating free cash flow). As of December 31, Mediacom's digital-cable service was available to approximately 1,540,000 digital-ready basic subscribers, or 97 percent of its entire basic subscriber base, and the company was marketing cable-modem service to roughly 2,320,00 data-ready homes, or 85 percent of its total homes passed. As of the same date, approximately 96% of its cable network was upgraded to 550MHz870MHz bandwidth capacity, and 91% of its homes passed were activated with two-way communications capability. By mid-2003, these figures both are expected to approximate 98%, with almost 95% of its subscribers served by only 50 of its remaining 176 master head-ends (which have decreased by over 55% during the course of 2002).

APPENDIX C

Accuracy of the Naive Bayes Classification Algorithm

This appendix presents within-sample performance measures of the Naive Bayes model by comparing the algorithmic classifications with the manual classifications. To evaluate the Naive Bayes within-sample accuracy, we use the entire manually coded training dataset to develop the Naive Bayes model, and then compare the model-predicted classifications with the manual classifications. We use the following terminology to estimate the model’s performance measures. TP represents “true positive” classifications. A “positive” outcome is the event of interest, which in our setting is the negative tone of debt analysts’ conflict discussions. FN represents “false negative” classifications. FP represents “false positive” classifications. TN represents “true negative” classifications.

Predicted by Algorithm	Actual (Manually Classified)		
	Negative	Non-Negative	
Negative	340(TP)	704(FP)	1,044
Non-Negative	184(FN)	4,705(TN)	4,889
	524	5,409	5,933

1. *Sensitivity* is $TP/(TP + FN)$, which in our case is $340/524 = 65$ percent. *Sensitivity* is directly related to the Type I error, as the $Sensitivity = 1 - \text{Type I error}$. Our Type I error is $184/524 = 35$ percent. In our case, this error represents the probability of the model incorrectly predicting a non-negative tone of conflict discussion when the tone is actually negative.
2. *Specificity* is $TN/(TN + FP)$, which in our case is $4,705/5,409 = 87$ percent. *Specificity* is directly related to the Type II error, as the $Specificity = 1 - \text{Type II error}$. Our Type II error is $704/5,409 = 13$ percent. In our case, this error represents the probability of the model incorrectly predicting a negative tone of conflict discussion when the tone is actually non-negative.
3. *Accuracy* is $(TP + TN)/(TP + TN + FP + FN)$, which in our case is $(340 + 4,705)/5,933 = 85$ percent. *Accuracy* is an aggregate measure that captures the probability that the tone will be correctly classified. It reflects both false positive and false negative errors.
4. *Positive Predictive Value* is $TP/(TP + FP)$, which in our case is $340/1,044 = 33$ percent. *Positive Predictive Value* reflects the percentage of true negative tones when the model predicts a negative tone.
5. *Negative Predictive Value* is $TN/(TN + FN)$, which in our case is $4,705/4,889 = 96$ percent. *Negative Predictive Value* reflects the percentage of true non-negative tones when the model predicts a non-negative tone.

Auditor Style and Financial Statement Comparability

Jere R. Francis

University of Missouri

Matthew L. Pinnuck

The University of Melbourne

Olena Watanabe

Iowa State University

ABSTRACT: The term “audit style” is used to characterize the unique set of internal working rules of each Big 4 audit firm for the implementation of auditing standards and the enforcement of GAAP within their clienteles. Audit style implies that two companies audited by the same Big 4 auditor, subject to the same audit style, are more likely to have comparable earnings than two firms audited by two different Big 4 firms with different styles. By comparable we mean that two firms in the same industry and year will have a more similar accruals and earnings structure. For a sample of U.S. companies for the period 1987 to 2011, we find evidence consistent with audit style increasing the comparability of reported earnings within a Big 4 auditor’s clientele.

Keywords: *earnings; comparability; Big 4 accounting firms.*

Data Availability: *All data are publicly available from the sources identified in the text.*

I. INTRODUCTION

Comparability is defined by the Financial Accounting Standards Board (FASB) as the quality of information that enables users to identify similarities and differences in the financial performance of two firms. The joint conceptual framework project of FASB and the International Accounting Standards Board (IASB) emphasizes that comparability is a basic property of financial information that enhances its usefulness (Financial Accounting Standards Board 2010). Indeed the FASB states that comparability in financial reporting is the primary reason

We thank the editors, Michael L. Ettredge and John Harry Evans III, and the two reviewers for their guidance and constructive suggestions. We also appreciate the comments of workshop participants at The University of Arizona, Bocconi University, Bond University, HEC Montreal, Laval University, Michigan State University, University of Missouri, Washington University in St. Louis, and the comments of participants at the 2011 American Accounting Association Annual Meeting and the 2011 International Symposium on Audit Research Annual Meeting. Professor Francis also holds an appointment as Distinguished Professor, University of Technology, Sydney.

Editor’s note: Accepted by Michael L. Ettredge.

Submitted: March 2012

Accepted: October 2013

Published Online: October 2013

for developing accounting standards (FASB 1980, para. 112), and the centrality of comparability is stressed in accounting textbooks, particularly financial statement analysis texts (Revsine, Collins, Johnson, and Mittelstaedt 2011; Phillips, R. Libby, and P. Libby 2013).

The primacy of comparability as a qualitative characteristic of accounting makes it important to understand the factors that give rise to this characteristic. The emerging research into the determinants of comparability has focused on the role of accounting standards such as the adoption of IFRS (Barth, Landsman, Lang, and Williams 2012; Lang, Maffett, and Owens 2010). However, accounting standards on their own do not fully determine financial reporting outcomes; economic agents and institutional incentives also play an important role (Ball, Robin, and Wu 2003; Leuz, Nanda, and Wysocki 2003). This motivates our investigation of the role that auditors play in the implementation of comparability in the United States. An advantage of studying financial statement comparability in a single-country setting is that we avoid the potential confounding effect of institutional differences in cross-country studies.

The concept and use of the word *comparability* differs in the literature. For the purpose of our study we define accounting comparability as the closeness of two firms' reported earnings due to the consistency with which rules are applied across firms. In our empirical context, this means that firm-pairs in the same industry and fiscal year, and therefore subject to the same general economic shocks, are expected to have a similar accruals and earnings structure, all things being equal. However, there are frictions in the interpretation, implementation, and enforcement of accounting standards that can reduce inter-company comparability. Our study focuses on the role of the auditor and, following Kothari, Ramanna, and Skinner (2010), we argue that each Big 4 audit firm has its own unique set of internal working rules that guide and standardize the auditor's application of auditing and accounting standards. These working rules give rise to what we term *audit style*, with the consequence that audit firms have systematic differences in their audit approaches, and in their interpretation and enforcement of accounting standards. As a result, we expect reported accruals and earnings to be more consistent and comparable within an audit firm's clientele than between audit firm clienteles. This leads to our main hypothesis: two companies audited by the same Big 4 auditor, and therefore subject to the same *audit style*, are more likely to have comparable earnings than two companies audited by two different Big 4 auditors and subject to different *audit styles*.¹

We measure accounting comparability in three ways. The first approach is to examine differences in year-specific total and abnormal accruals between pairs of firms in the same industry using the same Big 4 auditor versus firm-pairs with two different Big 4 auditors. The second approach measures the degree to which the earnings of a pair of firms in the same industry, and audited by the same Big 4 auditor, covary across time (Barth et al. 2012; De Franco, Kothari, and

¹ Some recent examples provide anecdotal evidence of auditor style effects. In a field study, Dichev, Graham, Harvey, and Rajgopal (2013, 32) cite CFOs on the effect of auditors on earnings:

"The big [accounting] firms are not passing authority downstream to the regional headquarters or onto the actual auditors like they used to . . . Interpretation of these rules in the accounting firms comes from high above now rather than from the field."

"... earlier you could work with your local accounting firm, your local partner and accomplish things. Now pretty much everything goes up to their think tank at national."

The implication is that the work of accounting firms is standardized across their clienteles, consistent with a "style" effect. Another example is from Blacconiere, Frederickson, Johnson, and Lewis (2011) who study firms making voluntary disclosures that disavow the reliability of mandated fair value information. Their descriptive statistics show that Ernst & Young (E&Y) clients are four times more likely to disavow the reliability of mandated fair value disclosures than are the clients of other national audit firms. E&Y included a disavowal as an illustrative disclosure in its SFAS No. 123 implementation guidance (E&Y 1995), and their clients used this wording almost verbatim in their disavowal disclosures.

Verdi 2011; Lang et al. 2010). The third approach follows the CEO/CFO style literature and uses an auditor fixed effects model to examine the commonality of accruals for auditor clienteles (Bamber, Jiang, and Wang 2010; Ge, Matsumoto, and Zhang 2011).

The primary tests are based on pairs of firm-year observations from Compustat in the same industry-year for the period 1987 to 2011. Consistent with our main hypothesis, we find that two firms in the same industry-year and audited by the same Big 4 auditor have more comparable earnings than two firms audited by two different Big 4 auditors. These results are consistent across the three empirical tests: pairs of firms in the same industry-year with the same Big 4 auditor have more similar total and abnormal accruals; firm-pairs with the same Big 4 auditor have a higher covariation in earnings over time; and auditor fixed effects are a statistically significant determinant of accruals. These results are robust to a set of controls for other factors that might create frictions or otherwise affect the comparability of accruals and earnings for firm-pairs. The findings are consistent with each Big 4 audit firm having a unique style that increases the comparability of earnings within its clientele relative to firm-pairs with two different Big 4 auditors. Economic magnitudes of these effects are reported in Section IX.

We also examine whether a pair of companies audited by the same Big 4 auditor will have more comparable earnings than a pair of companies audited by non-Big 4 auditors. The test is based on the argument that Big 4 auditors, due to their size and economies of scale, will have a greater capacity than non-Big 4 auditors to incur the fixed costs in developing standardized in-house rules for implementing auditing standards and the enforcement of GAAP. Consistent with this prediction we report evidence that Big 4 auditors have a greater effect on accounting comparability than non-Big 4 auditors.

The study makes several contributions to the literature. This is the first study to hypothesize and test the role of economic institutions within a country in the production of comparability. The existing debate and empirical evidence with regard to the production of comparability has focused almost exclusively on the role of standards themselves, especially FASB versus IFRS. Our study provides evidence that an economic institution—the auditor—is also an important factor in the production of financial statement comparability. Consistent with the joint FASB/IASB conceptual framework, our results suggest that accounting standards alone may not necessarily lead to comparability, but that the effects of standards are also dependent upon audit firms involved in the enforcement of GAAP. As such, we document a new channel through which auditor characteristics affect audited financial statements.

Second, we contribute to the debate on principles versus rules in the development of accounting standards by regulators. Kothari et al. (2010) argue that regulators should not be concerned with the potential for non-comparability if accounting standards are principle-based, because accountants and auditors who are involved in the day-to-day application of principles will develop “working rules” to standardize accounting practice. Our results suggest that this standardization process occurs within the clientele of one auditor; however, there are significant “style differences” between audit firms that reduces inter-auditor comparability.

Third, we contribute to the broader literature that examines the auditor’s role in the production of financial reports. This literature has mainly examined the role of auditing in curbing earnings management, which is related to the qualitative characteristic of “representational faithfulness” (FASB 2010). We show that the unique style of each Big 4 auditor affects the qualitative characteristic of comparability, and this is another source of variation within the Big 4 group of auditors. Our study and results are therefore related to Bamber et al. (2010) who report that individual corporate managers have their own individual style in choice of voluntary corporate financial disclosure, and Ge et al. (2011) who find that CFO style affects the choice of accounting policies. We extend the concept of unique styles in the production of financial reports from

individuals to accounting firms. Our finding is analogous to the finance literature that examines mutual funds and documents that funds have their own unique styles (Barberis and Shleifer 2003).

The next section develops the study's two hypotheses. The research design is presented in Section III, and sample selection and data are summarized in Section IV. Empirical results are reported in Sections V through VIII. Section IX discusses the economic magnitudes of the results, and Section X concludes the study.

II. HYPOTHESIS DEVELOPMENT

Two lines of research are relevant to this study: research that examines financial statement comparability, and research linking auditor characteristics with earnings attributes. While the importance of comparability has long been recognized by standard-setters, and discussed in the academic literature at a conceptual and normative level, there is much less empirical research on comparability. Recent empirical papers have emerged in response to the development of new methodologies to measure comparability, and to the widespread adoption of IFRS. These papers examine how the adoption of IFRS affects financial statement comparability, and how improved comparability affects decisions by investors. For example, Barth et al. (2012) examine the comparability of financial statements of non-U.S. firms that adopt IFRS with that of U.S. firms, and find that IFRS adoption by non-U.S. firms enhances their financial statement comparability with U.S. firms. Lang et al. (2010) examine changes in cross-country financial statement comparability around mandatory IFRS adoption and document that IFRS adoption increases comparability, measured as cross-country earnings co-movement. Other recent papers have examined whether comparability affects the decisions of participants in the capital market. De Franco et al. (2011) find that earnings comparability within an industry is positively related to analysts' following and accuracy, and negatively related to analysts' optimism and dispersion in earnings forecasts. Bradshaw, Miller, and Serafeim (2011) also study analysts and find that the commonality of accounting policy choices, their measure of comparability, affects analyst coverage and behavior. Last, DeFond, Hu, Hung, and Li (2011) show that mutual funds increase their foreign investment in countries with mandatory IFRS adoption, which they argue is due to improved cross-country earnings comparability.

Turning to the auditing literature, a large body of research has examined the association of auditor characteristics with clients' audited earnings. The seminal studies linking auditors and earnings attributes are Becker, DeFond, Jiambalvo, and Subramanyam (1998) and Francis, Maydew, and Sparks (1999), who document that Big 4 clients have smaller abnormal accruals than non-Big 4 clients. This stream of research has also examined other earnings attributes such as benchmark beating (Burgstahler and Dichev 1997; Frankel, Johnson, and Nelson 2002), accruals quality (Dechow and Dichev 2002; Doyle, Ge, and McVay 2007), and timely loss recognition (Basu 1997; Krishnan 2005). Francis (2004, 2011) reviews the empirical auditing literature and describes auditor characteristics associated with earnings quality, including the Big 4/non-Big 4 dichotomy, the auditor's industry expertise (Reichelt and Wang 2010), and engagement-specific factors such as client size (Reynolds and Francis 2000), auditor tenure (Johnson, Khurana, and Reynolds 2002), auditor-provided nonaudit services (Frankel et al. 2002), and the presence of audit firm alumni in executive positions of clients (Menon and Williams 2004).

We bring the comparability and audit research streams together to investigate the role of the auditor in comparability. While prior comparability research has examined the capital market effects from the global harmonization of standards, we know of no attempts to empirically document the role of economic agents such as auditors on financial statement comparability. Barth et al. (2012) recognize that accounting reports are the result of a complex interaction of the features of the financial reporting system that include accounting standards, their interpretation,

enforcement, and litigation, all of which can affect comparability. Apart from the actual accounting standards themselves, which are exogenously given, the auditor is actively involved in all of these features of the financial reporting system. The audit research literature, in turn, has focused on the role of the auditor in facilitating the reporting of high-quality earnings, with the primary emphasis on accruals quality and earnings management behavior. We extend this line of research to investigate the role the auditor plays in facilitating comparability.

Our argument is that each Big 4 audit firm has its own unique audit testing approach for implementing GAAS along with in-house working rules for interpreting and applying GAAP. The policies of each Big 4 firm will give rise to what we term an “audit style,” and we expect auditor style to have a systematic effect on clientele earnings. It is well-known that each Big 4 accounting firm has its own unique audit methodology and testing procedures. For example, Kinney (1986) classified the then Big 8 accounting firms based on their use of unstructured, intermediate, and structured audit technologies. While audit methods/procedures must comply with generally accepted audit standards (GAAS), the audit standards are themselves rather general in nature and much more principles-based than is U.S. GAAP. This means that each accounting firm must devise its own in-house working rules for the efficient and consistent implementation of GAAS across its client base (Cushing and Loebbecke 1986). Auditors also attempt to differentiate themselves from one another based on their methodologies. For example, in the 1980s there was a dichotomy between auditors that used a quantitative approach versus those that used a qualitative methodology (Kaplan, Williams, and Menon 1990). In the 1990s, KPMG promoted its “business risk” audit as an innovation (Bell, Marrs, Solomon, and Thomas 1997). These divergent practices are also illustrative of the kinds of technical innovation that Kothari et al. (2010) argue is more likely to occur when standards, in this case auditing standards, are principles-based rather than rule-based. The unique character of audit methodologies implies that each firm’s audit approach will systematically detect or not detect the same client errors, including GAAP implementation errors. The implication is that financial statements will be more similar for firm-pairs with the same auditor, *ceteris paribus*, than for firm-pairs with two different auditors each having different styles.²

Turning to style effects that arise from GAAP interpretation, it may not be as well-known, but each of the Big 4 accounting firms also has in-house rules for interpreting and implementing GAAP, just as it has for implementing auditing standards (GAAS). Kothari et al. (2010) develop the general argument that a principles-based approach to GAAP does not eliminate the role of “rules.” Instead a principles-based approach to GAAP will result in economic agents such as auditors developing in-house “working rules” for the consistent interpretation and implementation of standards. Kothari et al. (2010, 277) state that:

It is not likely to be cost effective for accountants and auditors to work with principles on a day-to-day basis. Authority on interpreting and implementing GAAP in an economy has to be delegated to thousands of rank-and-file accountants and auditors (for reasons of efficiency); this is possible only if working rules are formulated out of principles.

While there is more guidance in U.S. GAAP than in the relatively more principles-based international financial reporting standards (IFRS), the Big 4 accounting firms will still find it advantageous to develop their own in-house working rules because U.S. GAAP still requires

² Vera-Muñoz, Ho, and Chow (2006), Banker, Chang, and Kao (2002), Dowling (2009) report that accounting firms also use information technology systems to standardize the implementation of audit methodologies and that firms also standardize the documentation of audits using electronic templates for working papers that embed the audit firm’s methodology. Zerni (2012) identifies the following software products: KPMG’s KWorldTM, PricewaterhouseCoopers’ TeamAssetTM and KnowledgeCurveTM, and Ernst & Young’s KnowledgeWebTM.

considerable judgment in interpreting and implementing accounting standards. As a result, each Big 4 firm has its own in-house GAAP guide that is used internally by its auditors in the field.

To illustrate this point, we have identified the following current products developed by each Big 4 accounting firm for internal use by audit staff:

- Deloitte: Deloitte Technical Library (<http://www.deloitte.com/us/techlibrary>); Deloitte Roadmap (http://www.deloitte.com/view/en_US/us/Services/audit-enterprise-risk-services/Financial-Statement-Internal-Control-Audit/Accounting-Standards-Communications/980bef5fe91fb110VgnVCM100000ba42f00aRCRD.htm).
- Ernst & Young: Global Accounting and Auditing Information Tool (GAAIT) (<http://www.ey.com/GL/en/Services/Assurance/Assurance-Key-A-A-Guidance-On-Ernst—Young-Online—Global-Accounting—Auditing-Information-Tool>).
- KPMG: Accounting Research Online (<http://www.kpmg.com/global/en/topics/accounting-research-online/pages/default.aspx>).
- PricewaterhouseCoopers: Accounting Guides (<http://www.cfodirect.pwc.com/CFODirectWeb/Controller.jpf?NavCode=MSRA-777JJY>).

Each Big 4 firm explicitly states that their product represents a guide for the interpretation and application of GAAP. For example, Ernst & Young characterize their product as a global online resource for accounting and auditing standards, which represents Ernst & Young's interpretative guidance for U.S. GAAP, international GAAP, and other GAAP systems. Deloitte says its Technical Library provides interpretative guidance for GAAP, and KPMG says its guide contains regulatory pronouncements and KPMG's guidance on new pronouncements including illustrative examples to facilitate the practical application of standards.

While accounting firms originally developed these materials for internal use by their audit staff, they also provide some of the same information to their clients. In other words, clients are likely to be using their auditor's GAAP guidance products in preparing financial statements. Deloitte's Technical Library has a subscription price of \$2,000, and Ernst & Young's Global Accounting and Auditing Information Tool (GAAIT) has a base price of \$750. KPMG's Accounting Research Online is also available by subscription, and PricewaterhouseCoopers provides their accounting guides by subscription through its CFODirect network. In addition, our discussions with Big 4 practitioners reveal that when a complex accounting issue arises in the preparation of financial reports, the CFO will often seek guidance from the Technical Department of the firm's Big 4 auditor.³

The working rules of Big 4 auditors are an important mechanism through which GAAP is operationalized and implemented by both auditors and their clients, even within the United States with its arguably more explicit rule-based standards. The result is that two companies with the same Big 4 accounting firm as their auditor are more likely to interpret and implement GAAP in the same way, including the role of the auditor in enforcing GAAP and detecting GAAP misapplications for its clientele through the firm's standardized audit methodology.

Thus, if there are auditor style effects on their clienteles' financial reports, we should observe greater consistency in the financial statements of two companies in the same industry-year audited

³ The existence of in-house rules is supported by Acito, Burks, and Johnson (2009) who investigate accounting for operating leases and subsequent restatements for the misreporting of these leases. Acito et al. (2009, Table 4) use an auditor fixed effects model and finds systematic differences between auditor clienteles in the frequency of restatements that corrected lease accounting errors. This finding suggests that auditors had different GAAP interpretations with respect to lease accounting, which resulted in different rates of subsequent client restatements.

by the same Big 4 audit firm relative to firm-pairs with two different Big 4 audit firms where the effect of style is randomized away. Our first hypothesis, in alternative form, is:

H1: A pair of companies audited by the same Big 4 audit firm will have more comparable earnings than a pair of companies audited by two different Big 4 audit firms.

DeFond et al. (2011) point out that while comparability is the desired outcome of adopting a set of uniform accounting standards, uniformity alone does not necessarily result in comparability. In particular, the standards and in-house rules must also be faithfully implemented. This leads us to predict that the financial statements of a pair of firms with the same Big 4 auditor will have a greater comparability than financial statements of a pair of firms with the same non-Big 4 auditor. Because Big 4 auditors are larger, they have a greater capacity to incur the fixed-cost investments in audit programs and in-house rules for interpreting and implementing GAAP through “technical guidelines.” Second, because Big 4 auditors have a larger and more dispersed staff, they have greater incentive/need for staff controls than smaller firms. For these reasons we predict that Big 4 audit firms will have a greater style effect than non-Big 4 audit firms. Finally, higher quality auditors are more likely to correctly apply accounting standards, and prior research has found that non-Big 4 auditors are associated with lower-quality audited earnings (Teoh and Wong 1993; Becker et al. 1998; Francis et al. 1999).⁴ Because managers have flexibility in the application of accounting standards, comparability will be greater among companies with Big 4 auditors because the accounting standards are applied on a more consistent and correct basis. Thus, we predict increased financial statement comparability for firms with Big 4 auditors, and the second hypothesis in alternative form is:

H2: A pair of companies audited by the same Big 4 audit firm will have more comparable earnings than a pair of companies audited by the same non-Big 4 audit firm.

III. RESEARCH DESIGN

Prior earnings comparability research has typically analyzed either (1) cross-sectional similarities in the levels of contemporaneous measures (Joos and Lang 1994); or (2) the correlation of earnings (covariation) across time (Barth et al. 2012; De Franco et al. 2011).⁵ To test H1 we build on this research and use method (1) to test cross-sectional similarities of accruals for firm-pairs, and method (2) to test the correlation of earnings for firm-pairs across time. For robustness, and to provide a link to the CEO/CFO style literature, we use an auditor fixed effects model to test for systematic accounting similarities within audit firm clienteles.

Empirical Tests of H1

Differences in Accruals

Our first approach to testing accounting comparability is to examine the similarity or “closeness of accruals” for pairs of firms in the same industry, at a common point in time, conditional on audit firm. This approach is based on and extends prior comparability research that has examined similarities in cross-sectional levels of contemporaneous measures such as return on

⁴ There is also evidence that mid-tier audit firms with national practices provide higher quality audits than smaller non-Big 4 firms (Francis et al. 1999), and in the post-SOX period some studies report that audit quality differences have narrowed between the Big 4 firms and mid-tier firms (Boone, Khurana, and Raman 2010; Cassell, Giroux, Meyers, and Omer 2013).

⁵ An alternative approach uses the similarity of accounting policy choices (Bradshaw and Miller 2007; Bradshaw et al. 2011). We do not use this approach because of limited data availability on accounting method choices.

equity and price multiples to investigate cross-country convergence in firm-specific earnings multiples (Joos and Lang 1994; Land and Lang 2002). Our approach is conceptually similar except that we are examining auditors as an institutional factor giving rise to comparability and the convergence of earnings within a single country.

Our analysis examines accruals because it is the primary component of earnings that is subject to discretion and is the component through which economic agents such as auditors can most directly affect comparability. The premise is that two firms in the same industry and year, and audited by the same Big 4 auditor, are more likely to have the same type of accrual adjustments due to audit methodology, and to make the same set of accounting choices and judgments in implementing GAAP. Therefore, the accruals structure of these firms will be more similar than that of two firms with different auditors where the effect of audit style will be randomized away. We measure the similarity of accruals as follows:

$$Diff\_Total\_Accruals_{ijt} = abs(Total\_Accruals_{it} - Total\_Accruals_{jt}), \quad (1)$$

where $Diff\_Total\_Accruals_{ijt}$ is the absolute value of the difference between signed total accruals for firm-pairs in the same SIC two-digit industry classification in year t . We calculate this comparability metric for each firm i and firm j pairwise combination, for J firms in the same industry and fiscal year. We control for economic fundamentals and exogenous shocks because our analysis examines firm-pairs within the same industry and year where the accruals structure should be similar, *ceteris paribus*.

Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items ($IB - (OANCF - XIDOC)$), scaled by beginning of year total assets. We use the same approach to calculate differences in abnormal accruals, which we label $Abn\_Accr\_Diff_{ijt}$. Abnormal accruals are calculated using the Jones (1991) model of discretionary accruals, with control for contemporaneous performance (Kothari, Leone, and Wasley 2005). H1 predicts that financial statements for firm-pairs in the same industry-year with the same Big 4 auditor will report a more similar accrual structure and, therefore, will have smaller differences in both total accruals and abnormal accruals.

Earnings Covariation

A second way of measuring accounting comparability is the degree to which earnings for firm-pairs in the same industry covary over time (Barth et al. 2012; De Franco et al. 2011). This approach should be less subject to omitted variables that could affect the accrual-difference metric that may capture a number of properties other than comparability. The specific approach we employ follows De Franco et al. (2011; hereafter DKV), who measure comparability as the degree to which earnings for two firms in the same industry covary over time.⁶ Following DKV, we measure the level of covariance as the adjusted R^2 from the following regression:

⁶ DKV suggest two approaches for measuring comparability, an approach based on the similarity of the mapping of earnings to stock returns across firms, and an approach based on the covariation in earnings across firms. We use the latter approach in our study, the earnings covariation metric, because it is a more direct test of the arguments that underpin our hypothesis. As Lang et al. (2010) point out, the DKV comparability metric of mapping earnings to stock returns measures whether earnings are similarly capturing the underlying economics, while an earnings covariation metric captures anything that creates earnings similarity, regardless of whether the underlying economics are indeed similar. H1 in our study is underpinned by the concept of auditors having in-house rules that cause them to impose the same accounting choices on their clientele, regardless of the underlying economics, which leads to covariation in earnings. For this reason we use the earnings covariation metric and do not measure comparability based on the similarity of the mapping of earnings to stock returns because it is a fundamentally different construct.

$$Earnings_{iq} = \alpha_{0ij} + \alpha_{1ij}Earnings_{jq} + \varepsilon_{ijq}, \tag{2}$$

where *Earnings* is income before extraordinary items for firm *i* and firm *j* for quarter *q* scaled by average total assets of each firm. The model in Equation (2) is estimated over 16 consecutive quarters *q* for all unique pairs of firms in the same two-digit SIC industry. We measure the accounting comparability of firm *i* and firm *j* in Equation (2) as the adjusted *R*² from the regression, hereafter referred to as earnings comparability covariation, or *ECOMP_COV*. Higher values of *ECOMP_COV* indicate greater earnings comparability between firm-pairs. H1 predicts that earning covariation is greater for firm-pairs with the same Big 4 auditor.⁷

A potential limitation of the earnings covariation metric is that it does not explicitly control for economic shocks that are crucial to isolating accounting comparability. Following DKV, we address this issue in three ways. First, we perform all our analyses on firm-pairs within industry by year, thereby controlling for common economic fundamentals and shocks. Second, we control for contemporaneous cash flow covariation for firm-pairs, which is measured analogously to *ECOMP_COV*. Specifically, *CFO_COMP_COV* is created in an identical manner to *ECOMP_COV* except that in Equation (2) we replace *Earnings* with *CFO*, which is the ratio of quarterly cash flow from operations to the beginning of period market value. Finally, we control for contemporaneous monthly stock return covariation (*RET_COV*) for firm-pairs, which is measured analogously to *ECOMP_COV*. Stock returns will reflect all economic shocks and provide a further control for the effect of underlying economic fundamentals on accruals.

Regression Models

To examine the relation between accounting comparability and auditor style we estimate the following OLS regression models:

$$Diff\_Total\_Accruals_{ijt}(Abn\_Accr\_Diff_{ijt}) = \alpha_{0ij} + \alpha_1Same\_Big4_{jt} + \alpha_2Controls + \varepsilon_{ijt}. \tag{3}$$

$$ECOMP\_COV_{ijt} = \alpha_{0ij} + \alpha_1Same\_Big4_{jt} + \alpha_2CFO\_COMP\_COV + \alpha_3RET\_COV + \alpha_4Controls. \tag{4}$$

All tests are based on robust t-statistics that control for heteroscedasticity and with standard errors clustered at the firm level to control for potential non-independence (Petersen 2009). Results are robust to clustering by both firm and year, and to alternative clustering by unique firm-pairs.⁸

The regression models in Equations (3) and (4) are estimated using a sample of firm-pairs that have the same Big 4 auditor versus firm-pairs with two different Big 4 auditors. Because our data

⁷ The earnings covariation metric uses quarterly earnings data and the observable effects of the auditor on comparability may be weaker in quarterly data compared to annual earnings because only the annual earnings are subject to a full-scope audit (Brown and Pinello 2007). In contrast, interim earnings numbers for the first three quarters are simply reviewed by the auditor. Under the integrated reporting model, fourth quarter earnings must be equal to annual earnings less the sum of the first three quarters, so the fourth quarter earnings can be also noisy due to the implicit adjustments for prior-quarter estimation errors. While we can expect the auditor to influence the implementation of GAAP over the entire year, we do not expect the influence of “audit style” on earnings comparability to be as strong in quarterly data as it would be in fully audited annual earnings. This results in a design trade-off. While annual data would be more appropriate in our research context, it is not practical to use annual data given the long time-series required to calculate earnings covariation. Further, the testing framework requires that a firm have the same auditor over the time-series, which is also less likely when using a longer time-series of annual data.

⁸ The dependent variable in regression model (3) is truncated at 0, which under certain conditions can lead to biased OLS estimators. However, this is unlikely to be an issue in our setting as we have very few 0-value observations. Nevertheless we also estimate model (3) using a Tobit regression and the results remain unchanged.

extend to the era of the Big 8 accounting firms, each accounting firm is treated as unique for the years in which it exists in the sample data. For example, a firm audited by Price Waterhouse in 1997, and by PricewaterhouseCoopers in 1998, is treated as having a different auditor in each year. For convenience, we use the term “Big 4” to refer to all of these auditors.

To test H1 we use the indicator variable, *Same_Big4*, which is coded 1 if the auditor for a pair of firms is the same Big 4 firm, and 0 if auditors in a pair of firms are two different Big 4 auditors. In Equation (3) we predict a negative coefficient on *Same_Big4* because a lower value of *Diff_Total_Accruals* indicates a smaller difference in accruals and, hence, greater cross-sectional comparability of earnings. In contrast, in Equation (4), we predict a positive coefficient on *Same_Big4* because a larger value of *ECOMP_COV* indicates greater earnings covariation over time and, hence, greater time-series comparability of earnings.

Lang et al. (2010) point out that there is no theoretical or empirical guidance concerning appropriate control variables to include in a regression that explains earnings comparability, and they include control variables for size and market-to-book on the basis that these variables are widely used to capture many unobservable firm-specific characteristics. We include these variables but also control for a wider range of other variables identified in the literature that could result in the earnings between two firms being similar due to either economic fundamentals (e.g., volatility of operations) or the propensity to manage earnings (e.g., market-to-book ratio or leverage). The full set of control variables are: size, leverage, market-to-book, cash flow from operations, losses, standard deviation of sales, standard deviation of cash flows, and sales growth.⁹ Due to the absence of theory, we make no predictions as to what the signs of the coefficients on the control variables should be. In addition, in model (3) we also include the level of accruals as an independent variable to control for the finding from prior audit research that accrual levels differ across auditor clienteles (Becker et al. 1998; Francis et al. 1999). Therefore, regression model (3) examines whether auditors have an effect on the comparability of earnings that is incremental to their effect on accruals quality. We also include industry fixed effects at the two-digit SIC industry classification level as a further control for innate firm characteristics and potential omitted variables. Test variables and control variables are defined in Appendix A.

Since the dependent variable is calculated each year t for a pair of firms i and j , the control variables must also control for yearly characteristics of the firm-pair i and j . Following prior research that has used pairs of firms, we control for both the *levels* and *differences* in firm-pair characteristics (Francis, Huang, and Khurana 2009; De Franco et al. 2011). For the regressions that have *Diff_Total_Accruals_{ijt}* and *Abn_Accr_Diff_{ijt}* as the dependent variable we control for *levels* by entering the minimum value in each year t for the paired control variables for firm i and j .¹⁰ The *differences* are measured by the absolute values of yearly differences in the control variable values for firm i and firm j . For the regressions that have *ECOMP_COV* as the dependent variable we follow the same approach. However, for these regressions, the dependent variable is constructed from the correlation of earnings across 16 consecutive quarters for firm-pairs. We therefore estimate the average of each control variable for each firm i and each firm j across the corresponding 16 quarters. We use averages of each firm to construct the average minimum value, and differences in these averaged values are used to construct the difference metric.

⁹ In regression (3) using *Diff_Total_Accruals* as the dependent variable, we include cash flow from operations to control for cash flow fundamentals. For the regression in Equation (4) using *ECOMP_COV* as the dependent variable, we included the variable *CFO_COMP_COV* to control for cash flow fundamentals.

¹⁰ As an alternative control, we use the average value in each year t for the paired control variables for firm i and firm j . This shows similar results to entering the minimum of each firm pair and all conclusions remain the same.

IV. SAMPLE SELECTION AND DATA

Sample Construction

We begin with all non-missing observations for Compustat firms incorporated in the U.S. with data from 1987 through 2011. We use this period because we require “Cash Flows from Operations” as a control variable and also to measure total accruals (earnings minus cash flows), and these data became available in 1987. Following De Franco et al. (2011) we retain firms with fiscal year ends in March, June, September, and December. We retain only observations with at least 20 firms in a given two-digit industry, and delete firms with names containing “HOLDING,” “HOLDINGS,” “ADR,” “partnership,” “LP,” “LLP.” We also delete all firms that report negative total assets or total assets less than \$10 million, and firm-year observations in a year in which the firm switches its auditor. Further, the sample is constrained by the availability of “Cash Flows from Operations” as noted above. Finally, we winsorize all continuous variables at 1 percent and 99 percent.

We begin with the accruals-difference sample, in which all firms in an industry-year are exhaustively paired. For example, if there are three firms A, B, and C, then the firm-pairs would be A-B, B-C, and A-C. The earnings co-movement tests are based on a subsample of the accruals-differences sample. Specifically, we keep only those firms from the accruals-difference sample that have data for all variables for 16 consecutive quarters and that did not change auditors during the 16 quarter period. A firm-pair first enters the sample when 16 consecutive quarters of data first becomes available. To alleviate any concern in regard to the robustness of the t-statistics, we use firm-pair observations with non-overlapping four-year periods to mitigate concerns over non-independence of error terms.¹¹

During the sample period there were several audit firm mergers. For all our tests we only compare pairs of firms audited by the same auditor.¹² This has no effect on the accruals tests that are based on yearly cross-sectional data. However, it does result in a reduced sample for the *ECOMP_COV* analysis because this metric requires firm *i* to have the same exact audit firm across a four-year (16-quarter) period.

Descriptive Statistics

Table 1, Panel A reports descriptive statistics for all variables in the study. The test variable *Same_Big4* is coded 1 for 22.2 percent of the sample. For the accrual-difference metrics, the mean difference in total accruals (abnormal accruals) between firm-pairs is 11.3 (10.6) percent of total assets. The mean value of *ECOMP_COV* is an adjusted R^2 of 11.4 percent, similar to the 11.2 percent reported by De Franco et al. (2011). Panel B reports the correlation between *ECOMP_COV* and the accruals-difference metrics and shows a statistically negative association as would be predicted because larger accrual differences imply lower earnings comparability, although the magnitude of the correlations is only $r = 0.05$. The low correlation reflects: (1) the fact that accrual differences are measured after removing variation in earnings due to cash flows, while *ECOMP_COV* includes variation due to cash flows; (2) the accrual metric is a yearly cross-

¹¹ For example, if we estimate *ECOMP_COV_{ij}* for the firm-pair *i* and *j* using 16 consecutive quarterly observations from 1988 to 1991, then the next estimated observation of *ECOMP_COV_{ij}* for the firm-pair *i* and *j* that we would include in the sample is based on the 16 consecutive quarterly observations from 1992 to 1995.

¹² Touche Ross merged with Deloitte Haskins and Sells on December 4, 1989 to form Deloitte Touche (later renamed Deloitte); Coopers & Lybrand merged with Price Waterhouse on July 1, 1998 to form PricewaterhouseCoopers, and Arthur Young merged with Ernst & Whinney on October 1, 1989 to form Ernst & Young.

TABLE 1
Descriptive Statistics

Panel A: Summary Statistics of Variables

Variable	Min.	10%	Mean	Median	90%	Max.	STD
Dependent Variables							
<i>Diff_Total_Accruals</i>	0.000	0.013	0.113	0.077	0.270	0.974	0.113
<i>Abn_Accr_Diff</i>	0.000	0.013	0.106	0.075	0.249	0.848	0.101
<i>ECOMP_COV</i>	0.000	0.002	0.114	0.052	0.321	0.998	0.152
Test Variables							
<i>Both_Big4</i>	0.000	1.000	0.960	1.000	1.000	1.000	0.201
<i>Same_Big4</i>	0.000	0.000	0.222	0.000	1.000	1.000	0.416
Control Variables							
<i>TA_Min</i>	−0.641	−0.277	−0.127	−0.097	−0.023	0.356	0.116
<i>Abn_Accr_Min</i>	−0.463	−0.178	−0.051	−0.028	0.041	0.386	0.098
<i>TA_Avg</i>	−0.641	−0.177	−0.071	−0.061	0.020	0.356	0.086
<i>Abn_Accr_Avg</i>	−0.463	−0.087	0.002	0.006	0.085	0.386	0.073
<i>Size_Diff</i>	0.000	0.300	1.987	1.653	4.174	9.007	1.524
<i>Size_Min</i>	2.394	2.935	4.697	4.478	6.736	11.596	1.469
<i>Size_Avg</i>	2.394	3.883	5.691	5.574	7.652	11.596	1.451
<i>LEV_Diff</i>	0.000	0.009	0.216	0.160	0.489	1.316	0.214
<i>LEV_Min</i>	0.000	0.000	0.098	0.031	0.290	1.316	0.134
<i>LEV_Avg</i>	0.206	0.432	0.169	0.013	0.173	0.000	1.316
<i>MB_Diff</i>	0.000	0.151	1.696	0.967	4.171	18.446	2.091
<i>MB_Min</i>	−2.982	0.260	1.104	0.901	2.171	17.634	0.998
<i>MB_Avg</i>	−2.982	0.611	1.952	1.531	3.837	17.634	1.511
<i>CFO_Diff</i>	0.000	0.019	0.167	0.112	0.391	1.235	0.168
<i>CFO_Min</i>	−0.793	−0.295	−0.036	0.022	0.126	0.500	0.187
<i>CFO_Avg</i>	−0.793	−0.123	0.047	0.071	0.181	0.500	0.133
<i>LossProb_Diff</i>	0.000	0.000	0.335	0.250	0.813	1.000	0.296
<i>LossProb_Min</i>	0.000	0.000	0.167	0.063	0.563	1.000	0.245
<i>LossProb_Avg</i>	0.000	0.031	0.335	0.281	0.719	1.000	0.260
<i>STD_Sales_Diff</i>	0.000	1.358	95.146	18.756	243.841	2450.380	225.808
<i>STD_Sales_Min</i>	0.091	0.739	14.288	4.012	29.271	2450.550	46.549
<i>STD_Sales_Avg</i>	0.091	2.688	61.861	17.649	154.765	2450.550	130.283
<i>STD_CFO_Diff</i>	0.084	0.815	8.967	3.017	17.666	1253.810	26.996
<i>STD_CFO_Min</i>	0.000	0.858	54.564	10.356	140.579	1253.160	125.845
<i>STD_CFO_Avg</i>	0.095	2.079	36.249	10.447	90.407	1253.810	73.284
<i>STD_Sales_Grth_Diff</i>	0.000	0.017	0.788	0.115	1.262	2343.540	11.018
<i>STD_Sales_Grth_Min</i>	0.021	0.047	0.163	0.103	0.248	1115.240	0.975
<i>STD_Sales_Grth_Avg</i>	0.021	0.082	0.557	0.179	0.903	1203.550	5.700
<i>Ret_Diff</i>	0.000	0.076	0.683	0.436	1.493	8.084	0.843
<i>Ret_Min</i>	−0.955	−0.636	−0.133	−0.161	0.355	7.316	0.441
<i>Ret_Avg</i>	−0.955	−0.374	0.209	0.092	0.870	7.316	0.624

(continued on next page)

TABLE 1 (continued)

Panel B: Spearman Correlation Coefficients between Comparability Metrics

		I	II	III
I	<i>ECOMP_COV</i>	1.000		
II	<i>Diff_Total_Accruals</i>	−0.053 < 0.0001	1.000	
III	<i>Abn_Accr_Diff</i>	−0.049 < 0.0001	−0.701 < 0.0001	1.000

Panel A reports descriptive statistics for all 2,471,917 firm-pairs in the study for the sample period 1987–2011. Panel B reports correlations between the comparability metrics for firm-pairs in the study for the period 1987–2011. All variables are defined in the Appendix A.

sectional measure while *ECOMP_COV* measures comparability across time; and (3) it is well documented there is noise in any earnings-attribute metric.

V. ACCRUAL DIFFERENCES

Primary Results

Table 2 reports the estimation of firm-pair differences in total and abnormal accruals. In the test of H1, the coefficient on *Same_Big4* is negative and statistically significant at $p < 0.01$ (two-tailed) for both differences in total accruals and differences in abnormal accruals. This is consistent with greater similarity in the accruals structure for firm-pairs audited by the same Big 4 auditor, and supports H1 regarding the effect of audit firm style on accounting comparability. The signs of the coefficients on the control variables are generally as expected. *Accruals_Min*, *Size_Min*, *CFO_Min* all have negative coefficients. As the minimum level of accruals, firm size, and cash flows increases in magnitude, firm-pairs are likely be more similar and therefore are more likely to have a similar accruals structure and smaller differences in accruals.

Auditor Changes

The next analysis is limited to subsamples where there is an auditor change for the firm-pair. We begin with the notion that if a pair of firms has different auditors and one of the firms changes auditor to have the same auditor as the other firm, then after the switch we should observe a more similar accruals structure resulting in smaller differences because the two firms are now subject to the style effects of the same auditor. To test this expectation, we re-estimate the regression model in Equation (3) for a subsample of firm-pairs that have switched from having two different Big 4 auditors (pre-switch) to having the same Big 4 auditor post-switch:

$$Diff\_Total\_Accruals_{ijt}(Abn\_Accr\_Diff_{ijt}) = \alpha_{0ij} + \alpha_1 S\_Switch_{jt} + \alpha_2 Controls + \varepsilon_{ijt}, \tag{5}$$

where *S_Switch* is an indicator variable that takes the value of 1 in the test years following the switch (same auditor), and the value of 0 in the benchmark years prior to switch (different auditors). Therefore the indicator variable *S_Switch* compares the differences in accruals for the same pair of firms, before and after the switch. We predict a negative coefficient if switching to the same auditor decreases the accrual differences for a pair of firms. The dependent variable and control variables are the same as previously described. We estimate this regression across three alternative periods of increasing length before and after the switch.

TABLE 2
OLS Results for Accruals Comparability Tests
Y = Diff. Signed Total Accruals Y = Diff. Signed Abnormal Accruals

Variable	Coeff.	t-stat.	p-value	Coeff.	t-stat.	p-value
Intercept	0.073	17.76	0.000***	0.104	18.99	0.000***
Same_Big4	−0.001	−4.68	0.000***	−0.001	−3.26	0.001***
Accruals_Min	−0.723	−253.14	0.000***	−0.713	−303.72	0.000***
Size_Diff	−0.004	−20.76	0.000***	−0.002	−14.95	0.000***
Size_Min	−0.007	−21.26	0.000***	−0.004	−15.90	0.000***
LEV_Diff	0.000	0.01	0.990	0.003	2.78	0.006***
LEV_Min	−0.013	−6.45	0.000***	−0.002	−1.46	0.144
MB_Diff	0.001	9.11	0.000***	0.000	2.38	0.018**
MB_Min	0.003	8.25	0.000***	0.000	−0.07	0.944
CFO_Diff	−0.060	−15.71	0.000***	−0.033	−11.95	0.000***
CFO_Min	−0.181	−31.57	0.000***	−0.184	−40.20	0.000***
LossProb_Diff	−0.033	−28.83	0.000***	−0.022	−23.28	0.000***
LossProb_Min	−0.067	−28.82	0.000***	−0.027	−14.10	0.000***
STD_Sales_Diff	0.000	4.06	0.000***	0.000	2.34	0.019**
STD_Sales_Min	0.000	0.36	0.720	0.000	−0.60	0.547
STD_CFO_Diff	0.000	3.66	0.000***	0.000	3.10	0.002***
STD_CFO_Min	0.000	3.33	0.001***	0.000	4.03	0.000***
STD_Sales_Grth_Diff	0.000	3.30	0.001***	0.000	−0.74	0.457
STD_Sales_Grth_Min	0.000	1.38	0.167	0.000	1.09	0.278
Industry FE	Yes			Yes		
R ²	0.560			0.537		
n	2,471,917			2,471,917		

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively. All p-values are based on robust standard errors clustered at the firm level. There are 6,044 unique firms/clusters for the t-tests. Table 2 reports an OLS regression that examines the impact of having the same Big 4 auditor on the pairwise financial statement comparability between firms, where comparability is measured based on differences in accruals between firm *i* and firm *j*. The dependent variables are the absolute value of the differences between firm *i* and firm *j* in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items, all scaled by beginning of year total assets. Abnormal accruals are calculated using the Jones (1991) model of abnormal accruals as modified by Kothari et al. (2005). The test variable *Same_Big4* is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are two different Big 4 auditors. Control variables are defined in Appendix A.

We begin by comparing accruals differences two years before (*t*−2 and *t*−1), and three years after the switch *t*0, *t*+1, and *t*+2, where the first year of the new auditor is denoted *t*0.¹³ We then expand this to three years prior to the switch (*t*−3 through *t*−1) and four years after the switch (*t*0 through *t*+3), and finally to four years before and five years after the switch, *t*−4 to *t*+4. As reported in Panel A, Table 3, the results across all sub-periods examined show that the predicted coefficient on *S_Switch* is negative as expected, and statistically significant at *p* < 0.01, except for

¹³ We allow a three-year transitional period for the new auditor to imprint their style on the grounds that it will take more than one year to do so. However, in untabulated tests we find the same result if we compare just the last year before the switch (two different audit firms) and the first year after the switch (the same audit firm).

TABLE 3
Accruals Comparability for Subsample of Auditor Switches

Panel A: Pair of Firms Switch to Have the Same Big 4 Auditor

	Variable	Coeff.	t-stat.	n
<i>t</i> −2, <i>t</i> +2				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>S_Switch</i>	−0.003	−2.93***	66,405
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>S_Switch</i>	−0.003	−3.39***	66,405
<i>t</i> −3, <i>t</i> +3				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>S_Switch</i>	−0.005	−4.86***	49,630
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>S_Switch</i>	−0.004	−3.72***	49,630
<i>t</i> −4, <i>t</i> +4				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>S_Switch</i>	−0.003	−2.59***	35,542
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>S_Switch</i>	−0.002	−1.93*	35,542

Panel B: Pair of Firms Switch to Different Big 4 Auditors

	Variable	Coeff.	t-stat.	n
<i>t</i> −2, <i>t</i> +2				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>D_Switch</i>	−0.001	−0.82	47,571
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>D_Switch</i>	0.000	0.07	47,571
<i>t</i> −3, <i>t</i> +3				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>D_Switch</i>	−0.002	−1.45	33,026
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>D_Switch</i>	−0.001	−0.41	33,026
<i>t</i> −4, <i>t</i> +4				
<i>Y</i> = <i>Diff_Total_Accruals</i>	<i>D_Switch</i>	0.002	1.14	20,925
<i>Y</i> = <i>Abn_Accr_Diff</i>	<i>D_Switch</i>	0.001	0.86	20,925

(continued on next page)

Abn_Accr_Diff in the period *t*−4 to *t*+4, which is significant at *p* < 0.10. These results for years around auditor changes provide compelling evidence that auditor style has an effect on clientele accruals, and that a change to the same auditor leads to more similar accruals.

For completeness, we also examine the situation where firm-pairs have the same auditor, and one of them changes auditors so that the firm pair now has two different auditors. We estimate the same regression as in Equation (5) except we include an indicator variable *D_Switch* that takes the value of 1 in the test years following the switch to different auditors, and the value of 0 in the benchmark years prior to switch when the pair of companies had the same auditor. The results are reported in Table 3, Panel B. Across all test periods, the coefficient on *D_Switch* is not statistically significant from 0. Therefore, there is no evidence that a change to different auditors reduced comparability in the post-switch period. We have no explanation for why the evidence from the two sets of switching tests in Table 3 is not symmetrical.

The literature on auditor switches has proposed a number of reasons for firms switching auditors. One possibility is that the motive for an auditor change could give rise to an omitted variable that biases the accrual-difference test. However, this bias should affect auditor changes in both directions, those that give rise to a firm pair with the same auditor (*S_Switch*) and those auditor changes that give rise to a firm pair with the different auditors (*D_Switch*). Therefore, our first approach to controlling for this possible bias is to predict that an *S_Switch* decreases accrual-differences by a greater amount than a *D_Switch*. By directly comparing the relative accrual-

TABLE 3 (continued)

Panel C: Statistical Tests For the Difference between *S_Switch* and *D_Switch* Coefficients

	Difference	t-stat.	p-value
<i>t</i> −2, <i>t</i> +2			
<i>Y</i> = <i>Diff_Total_Accruals</i>	−0.002	1.07	0.285
<i>Y</i> = <i>Abn_Accr_Diff</i>	−0.003	2.16	0.031
<i>t</i> −3, <i>t</i> +3			
<i>Y</i> = <i>Diff_Total_Accruals</i>	−0.003	1.99	0.046
<i>Y</i> = <i>Abn_Accr_Diff</i>	−0.003	2.12	0.034
<i>t</i> −4, <i>t</i> +4			
<i>Y</i> = <i>Diff_Total_Accruals</i>	−0.005	2.48	0.013
<i>Y</i> = <i>Abn_Accr_Diff</i>	−0.003	2.00	0.046

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively. All p-values are based on robust standard errors clustered at the firm level. Panel A reports an OLS regression for a sample of firm-pairs for which one firm has changed auditors resulting in the firm-pair switching from having different Big 4 auditors to having the same Big 4 auditor. The test variable *S_Switch* is coded 1 in the years after the switch when both auditors in a pair of firms have the same Big 4 auditor, and 0 for the years prior to the switch when auditors in a pair are two different Big 4 auditors. Therefore *Switch* estimates the change in accrual-differences between a pair of firms before and after the switch from having different to having the same auditor. Panel B reports an OLS regression for a sample of firm-pairs that have switched from having the same Big 4 auditor to having different Big 4 auditors. The test variable *D_Switch* is coded 1 in the years after the switch when both auditors in a pair of firms have different Big 4 auditors, and 0 for the years prior to the switch when auditors in a pair are the same Big 4 auditors. The dependent variables are differences in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. For brevity, control variables are not reported.

differences of an *S_Switch* versus a *D_Switch*, we control for any systematic bias associated with the act of auditor switching, *per se*. We find across all the three test periods that *S_Switch* results in smaller accrual-differences than occurs in a *D_Switch*. These results are reported in Table 3, Panel C, which is at least suggestive that switching to the same auditor leads to relatively more comparability than switching to different auditors.

One of the often-cited reasons for auditor switches is opportunistic opinion shopping, although academic research has found little or no evidence of this (Chow and Rice 1982; Smith 1986; Johnson and Lys 1990; Francis and Wilson 1988; DeFond 1992; Lennox 2000). To control for the potential effect of opinion shopping we remove firms from our sample that had a qualified audit opinion in the year prior to the switch and re-estimate the auditor switch tests. The results show across all sub-periods examined that the coefficient on *S_Switch* is negative as predicted, and statistically significant. There is also evidence that auditor changes are associated with increased litigation risk and financial distress, which could bias the tests in Table 3 (DeFond and Subramanyam 1998; Shu 2000). DeFond and Subramanyam (1998) proxy for litigation risk and distress by using a variable for audit report qualifications in the prior year, which we control for in the models. As an alternative approach to jointly proxy for poor financial performance and litigation risk, we eliminate firms with extreme values of earnings (DeFond and Subramanyam 1998).¹⁴ The

¹⁴ Extreme values are controlled by including an indicator variable coded 1 if firm-year observations are in the upper (lower) 5 percent of the distribution of return on assets.

results for this subsample remain unchanged from the full sample; specifically, across all sub-periods examined, the coefficient on S_Switch is negative as predicted, and statistically significant, and D_Switch remains insignificant.¹⁵

Other Robustness Tests

The first robustness test is based on the notion that auditors can impose comparability on the accruals component of earnings, but not the cash flow component. Therefore, we re-estimate the regression model in Equation (3) using firm-pair differences in cash flows from operations as the dependent variable, and expect the auditor test variable to be insignificant in this analysis. Untabulated results confirm that the coefficient on $Same\_Big4$ is not statistically different from zero at the $p = 0.10$ level. This provides additional evidence that audit style affects earnings comparability only through the accruals component of earnings, which is more subjective and discretionary than the cash flow component.

We have argued that auditors impose comparability through their in-house policies for the implementation of GAAS and GAAP. These policies are more likely to exist for routine or typical transactions than for non-routine transitory transactions that, by definition, are more idiosyncratic in nature. Thus, there is little advantage in having standardized in-house rules for atypical, non-routine transactions. To test this we create a subsample of firm-pairs that report special items, and compute the absolute value of differences in the signed value of reported special items for firm-pairs i and j . This variable is more likely to reflect non-routine transitory transactions and therefore is less likely to be affected by audit style. Consistent with our expectation, when we estimate our baseline regression (3) with special items as the dependent variable, the coefficient on $Same\_Big4$ is not statistically significant at the $p = 0.10$ level.

Next, we address the concern that the control variables may not effectively control for differences in economic fundamentals, particularly when differences in economic fundamentals for firm i and firm j are large. Therefore, we remove all firm-pairs from the sample where the difference in sales revenue between firm i and firm j is greater than 20 percent. Untabulated results show that the coefficient on $Same\_Big4$ is negative and statistically significant at the $p < 0.01$ level for differences in total accruals and differences in abnormal accruals, which indicates that size differences in firm-pairs do not affect the results.

VI. EARNINGS COVARIATION

The test of H1 using earnings covariation ($ECOMP\_COV$) is reported in Table 4, using the model in Equation (4). The coefficient on the test variable $Same\_Big4$ is positive and statistically significant at the $p < 0.01$ level, consistent with the earnings of a pair of firms audited by the same Big 4 auditor being more comparable over time than the earnings of a pair of firms audited by two different Big 4 auditors.

Signs of the coefficients on the control variables in Table 4 are largely as expected. The positive coefficient on CFO_COMP_COV indicates that as cash flow from operations for two firms becomes more highly correlated, so do their earnings. Similarly, firm-pairs with more similar stock return covariation (RET_COV) have more correlated earnings, which is consistent with the mapping of earnings to stock returns. The negative coefficients on $Size\_Diff$ and $LossProb\_Diff$ are consistent with less earnings covariation when there is a greater difference in the size of two firms or in probability of reporting a loss for the two firms. Finally, the negative coefficient on

¹⁵ Another reason proposed for auditor switches is to align cross-temporal changes in client characteristics and differences in audit firm cost structure, resulting in growing firms switching to larger auditors (Johnson and Lys 1990). This motivation for switching does not apply to our sample, which is restricted to Big 4 auditors.

TABLE 4
OLS Results for *ECOMP_COV* Comparability Metric

Variable	Coeff.	t-stat.	p-value
Intercept	0.094	20.27	0.000***
<i>Same_Big4</i>	0.002	3.34	0.001***
<i>Size_Diff</i>	0.000	−1.18	0.238
<i>Size_Min</i>	0.002	5.46	0.000***
<i>LEV_Diff</i>	−0.005	−2.59	0.010***
<i>LEV_Min</i>	0.007	1.80	0.072*
<i>MB_Diff</i>	0.003	6.11	0.000***
<i>MB_Min</i>	0.000	2.23	0.026**
<i>LossProb_Diff</i>	−0.013	−11.27	0.000***
<i>LossProb_Min</i>	−0.003	−2.31	0.021**
<i>STD_Sales_Diff</i>	0.000	3.80	0.000***
<i>STD_Sales_Min</i>	0.000	2.72	0.007***
<i>STD_CFO_Diff</i>	0.000	−3.59	0.000***
<i>STD_CFO_Min</i>	0.000	−4.13	0.000***
<i>STD_Sales_Grth_Diff</i>	−0.001	−4.55	0.000***
<i>STD_Sales_Grth_Min</i>	−0.016	−8.06	0.000***
<i>CFO_COMP_COV</i>	0.084	22.75	0.000***
<i>RET_COV</i>	0.042	15.72	0.000***
Industry Fixed Effects	Yes		
R ²	0.014		
n	676,952		

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively. All p-values are based on robust standard errors clustered at the firm level. There are 5,078 unique firms/clusters for the t-tests. Table 4 reports an OLS regression in which the dependent variable *ECOMP_COV* is a pairwise measure of financial statement comparability based on earnings co-movement between firm *i* and firm *j* across 16 quarters. The test variable is *Same_Big4* that is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are two different Big 4 auditors. Control variables are defined in Appendix A.

STD_CFO_Min is consistent with a greater variation in cash flows leading to less earnings comparability.

VII. AUDITOR FIXED EFFECTS

An alternative approach to test for the effect of audit style on comparability is to determine if auditor fixed effects explain the level of accruals reported by each individual firm *i*. Rather than examining firm-pairs, this approach uses the level of accruals reported by each individual firm *i* as the dependent variable, and examines whether auditor fixed effects explain significant cross-sectional variation in accruals using the following OLS regression:

$$Total\_Accruals_{it}(\text{or } Abnormal\_Accruals_{it}) = \alpha_{0i} + \gamma + \alpha_1 Controls_{it} + \varepsilon_{it}, \tag{6}$$

where *Total_Accruals_{it}* and *Abnormal_Accruals_{it}* of firm *i* in year *t* are as previously defined, and γ is the set of individual auditor fixed effect coefficients. The control variables are firm size, market-to-book, leverage, cash flows from operations, loss, standard deviation of sales, standard deviation of cash flows from operations, and standard deviation of sales growth for each firm *i* and

year t . We also include industry fixed effects to control for potential omitted variables bias (Greene 2011). Thus, our estimated auditor fixed effects model captures the incremental effect of each auditor *after controlling for firm-specific variables and industry-wide fixed effects*. We predict that an F-test will reject the null hypothesis that coefficients of the individual auditor fixed effects are the same in the regression model. This approach to testing for audit style is conceptually similar to the stream of research that examines whether managers have individual styles. The seminal paper by Bertrand and Schoar (2003) finds that manager fixed effects are significant for a wide range of corporate decisions and interpret this result to be consistent with general differences in “style” across managers. Bamber et al. (2010) and Ge et al. (2011) extend this research to the accounting effects of style by CEOs and CFOs.

In untabulated results, the F-statistic testing the equality of auditor fixed effects is 14.67 for a regression with *Total_Accruals* as the dependent variable, and 4.07 for a regression with *Abnormal_Accruals* as the dependent variable, both significant at the $p < 0.01$ level. These results reject the null hypothesis that each auditor affects accruals in the identical manner, providing additional evidence that audit firms have individual styles in their approach to the interpretation and enforcement of GAAP that they impose on clients. A significant individual auditor fixed effect in the presence of an extensive set of controls and industry fixed effects requires that the audit firm exerts a systematic effect on accruals that is above or below the mean relative to other audit firms. In other words, an audit firm has a style that is unique and common across its client base, and that is systematically different from that of one or more other auditors.¹⁶

We extend this approach by testing individual line items in the financial statements. We focus on reported line items where there is some likelihood of differences in reported amounts due to inherent judgment that is where auditor style effects would be most expected. The accounts we examine are: inventory, capitalized leases, and pension expense. We first examine inventory because it is well documented in the audit literature that inventory is an asset subject to inherent risk that requires specific audit procedures. Consistent with this, a large volume of audit fee literature consistently finds inventory is positively related to audit fees (Hay, Knechel, and Wong 2006). Accordingly, we predict any style differences between auditors are likely to be evident in this account. In regard to capitalized leases, Acito et al. (2009) report that beginning in late 2004 through mid-2006, more than 250 U.S. firms disclosed that the operating lease accounting methods they had been using violated GAAP and that the violations were similar in nature. This suggests that there may be systematically different interpretations in the application of the rules or the materiality thresholds in applying the lease accounting standards (FASB 1976, 1988). We also examine the expected rate of return for pension assets as there is substantial flexibility in deciding the assumption that, in turn, can materially affect pension expense (Comprix and Muller 2006; Picconi 2006; Ge et al. 2011).

In untabulated results, the F-statistics for auditor fixed effects are 8.35, 10.16, and 44.28 for regressions with *inventory* (cost of goods sold/average inventory), *lease capitalization* (capitalized leases/total assets), and *pension expense* (pension interest rate assumption), respectively. These F-tests are all significant at the $p < 0.01$ level, so we reject the null hypothesis that auditors are associated with the accounting for inventory, leases, and pension expense in exactly the same manner. This analysis provides additional evidence that audit firms have individual styles in their approach to the interpretation and enforcement of GAAP in clients' financial reports, and reinforces

¹⁶ The F-statistic only shows that at least one firm is different from the other auditors and not that each auditor is different from all other auditors. To provide more robust evidence, we conduct a Wald test of equality of coefficients between alternate combinations of two auditors (i.e., PwC versus KPMG; Ernst & Young versus Deloitte, and so on). There are 21 unique combinations of auditor pairs of which 18 are statistically different from each other, which indicates there are style differences between nearly all of the Big N auditor pairs.

the findings in Table 2 and Table 3 concerning the similarity of accruals for firm-pairs in the same industry-year.

In summary, the results from accruals differences, earnings covariation, and auditor fixed effects are all consistent with the prediction in H1 that audit style affects accounting comparability. Specifically, firm-pairs in the same industry-year with the same Big 4 auditor have greater cross-sectional similarity in accruals and greater earnings covariation over time, and the auditor fixed effects tests show that there are systematic clientele differences in accruals and financial statement line items.¹⁷

Self-Selection

Firms are not randomly assigned to an auditor, rather, they choose one. To the extent that the reasons for choosing an auditor are not randomly distributed across firms, but rather are correlated across firm-pairs, self-selection could have an implication for the interpretation of the results. However, the test of auditor changes in Table 3 provides compelling evidence that audit style is the causal mechanism we observe in the data. In addition, the use of industry fixed effects is a standard econometric procedure to control for omitted variables that is a source of selection bias (Greene 2011).

There are other reasons to believe self-selection is not a significant issue in this study. First, the vast bulk of the theory and the empirical evidence associated with auditor choice has been associated with the choice between a Big N and a non-Big N auditor. This self-selection decision is not relevant to our primary H1 that examines comparability within the population of Big N auditors. Second, to the extent self-selection is driven by the choice of an auditor because they have a specific audit methodology and/or interpretation of accounting standards that suits the firm, this selection-motivation is consistent with H1.

Notwithstanding these observations, we conduct some tests to alleviate self-selection concerns. We begin by considering motives for a firm choosing an auditor that could be associated with a firm's accounting production function. Some papers have documented evidence consistent with auditors being industry specialists. It is therefore possible that two firms with similar accounting production functions could choose the same auditor because they have a similar demand for an industry specialist. To guard against this we remove from the sample firm-pairs that had an auditor that meets the typical definition of an auditor specialist in the literature.¹⁸ For this reduced sample, the coefficient on *Same_Big4* for accrual-differences (earnings covariation) metric remains negative (positive) and statistically significant at the 0.01 level. Another possibility is that due to proprietary information, two competitors in the same industry do not want to share the same auditor. To the extent competitors share a similar accounting production function, this type of self-selection creates a bias against finding a result as similar firms are selecting different auditors. Nevertheless, we address this self-selection issue by following prior literature and use measures of industry competition based on the Herfindahl index to proxy for proprietary costs of disclosure (e.g., Harris 1998; Botosan and Stanford 2005; Berger and Hann 2007). We calculate the Herfindahl index using sales by two-digit industry-year and assign an index value to a firm-pair per industry-year. To control for the similarity that might be induced by close competition, we remove yearly firm-pairs

¹⁷ In an untabulated analysis we also looked at the closeness of the *C_SCORE* metric for firm-pairs (Khan and Watts 2009). Consistent with the above results, we find that *C_SCORE*s are closer in value for firm-pairs with the same Big 4 auditor than for firm-pairs with two different auditors, which is evidence that auditors have a systematic style with respect to the level of accounting conservatism they impose on their clientele.

¹⁸ Consistent with the literature, an industry specialist is defined as an auditor that audits more than 30 percent of the total assets of an industry or, as an alternate definition, an industry specialist is an auditor that audits the most sales in an industry.

in the highest decile of the Herfindahl index and re-estimate the regression models. The results in Table 2 and Table 4 are unchanged. The coefficient on *Same_Big4* for the accrual-difference (earnings covariation) metric remains negative (positive) and statistically significant at the 0.01 level.

VIII. BIG 4 VERSUS NON-BIG 4 AUDITORS

This section reports the test of H2, which predicts greater earnings comparability for firm-pairs with the same Big 4 auditor than firm-pairs with the same non-Big 4 auditor. As a starting point we examine if there are non-Big 4 auditor style effects on the comparability of earnings. We re-estimate the model in Equation (3) for a subsample of firm-pairs with non-Big 4 auditors, and create an indicator variable *Same_NonBig4* that is coded 1 if two firms are both audited by the same non-Big 4 auditor, and 0 if they are audited by two different non-Big 4 auditors. The results are reported in Table 5, Panel A. For parsimony we do not report the control variables.

The results show a negative and statistically significant coefficient on the variable *Same_NonBig4* for total accruals but not abnormal accruals. The sign for the earning co-movement metric (*ECOMP_COV*) is also significant but in the opposite direction to that predicted for greater comparability. Therefore, there is no consistent evidence that two companies audited by the same non-Big 4 auditor have more comparable earnings than a pair of companies audited by two different non-Big 4 auditors. We also present the results just for a subsample of firm-pairs audited by mid-tier auditors. We examine mid-tier auditors as it is possible they have a greater investment in in-house programs than other smaller non-Big 4 auditors. The variable *Same_MidTier* is coded 1 if two firms are both audited by the same mid-tier auditor, and 0 if they are audited by two different mid-tier auditors, where the mid-tier auditors are BDO Seidman, Grant Thornton, and McGladrey and Pullen. The results reported in Table 5, Panel B show a negative and statistically significant coefficient on the variable *Same_MidTier* for total accruals (abnormal accruals are weakly significant at the 10 percent level, one-tailed), and earnings covariation is not significant. Overall we conclude there is only weak and inconsistent evidence that having the same non-Big 4 auditor affects the comparability of earnings.

We now turn to a formal test of H2. We first approach this by constructing a subsample of firm-pairs that either have the same Big 4 auditor or the same non-Big 4 auditor. We then re-estimate the model in Equation (3) with the indicator variable *Same_Big4* that is coded 1 if two firms are both audited by the same Big 4 auditor, and 0 if they are audited by the same non-Big 4 auditor. The results reported in Table 5, Panel C show a negative and statistically significant coefficient on the variable *Same_Big4* for total accruals, but a statistically insignificant coefficient for abnormal accruals. For *ECOMP_COV* the coefficient is positive (as predicted) and weakly statistically significant at the 10 percent level (one-tailed). Therefore, there is some evidence consistent with Big 4 auditors having a greater effect on earnings comparability than non-Big 4 auditors. Clients of Big 4 and non-Big 4 auditors can differ in economic fundamentals for which an OLS regression may not adequately control. To address this, we examine a subsample of firm-pairs in which the size of each firm must be between the smallest firm audited by a Big 4 auditor and the largest firm audited by a non-Big 4 auditor. The tests in Table 5, Panel D show a negative and statistically significant coefficient on the variable *Same_Big4* for total accruals, and marginally statistically significant coefficients for abnormal accruals and earnings covariation (10 percent level, one-tailed). In summary, there is support for H2 that Big 4 auditors have greater accounting comparability within their clienteles than occurs for non-Big 4 auditors.

TABLE 5

Impact of Big 4 versus Non-Big 4 Auditor on the Comparability of Earnings

Panel A: Same Non-Big 4 versus Different Non-Big 4

Variable	Diff_Total_Accruals		Dep. Var. = Abn_Accr_Diff		ECOMP_COV	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Same_NonBig4	−0.004	−2.28**	−0.001	−1.05	−0.004	−1.94*
n	28,615					

Panel B: Same Mid-Tier versus Different Mid-Tier

Variable	Diff_Total_Accruals		Dep. Var. = Abn_Accr_Diff		ECOMP_COV	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Same_MidTier	−0.003	−2.12**	−0.002	−1.38	0.001	0.87
n	22,809					

Panel C: Same Big 4 versus Same Non-Big 4

Variable	Diff_Total_Accruals		Dep. Var. = Abn_Accr_Diff		ECOMP_COV	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Same_Big4	−0.003	−1.78*	−0.002	−1.26	0.003	1.32
n	559,516					

Panel D: Same Big 4 versus Same Non-Big 4 for Subsample of Size-Matched Sample

Variable	Diff_Total_Accruals		Dep. Var. = Abn_Accr_Diff		ECOMP_COV	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Same_Big4	−0.003	−1.83*	−0.002	−1.32	0.003	1.25
n	474,352					

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels (two-tailed), respectively. All p-values are based on robust standard errors clustered at the firm level. Panels A and B report an OLS regression that examines the impact of non-Big 4 auditors on comparability. Panels C and D report an OLS regression that examines the difference in impact on comparability of earnings between a Big 4 auditor versus a non-Big 4 auditor. Comparability is measured based on differences in accruals between firm *i* and firm *j*. The dependent variables are the absolute value of the differences between firm *i* and firm *j* in total accruals, *Diff_Total_Accruals*, and differences in abnormal accruals, *Abn_Accr_Diff*. The test variable *Same_NonBig4* in Panel A is coded 1 if both auditors in a pair of firms are the same non-Big 4 firm, and 0 if auditors in a pair are two different non-Big 4 auditors. The test variable *Same_MidTier* in Panel B is coded 1 if both auditors in a pair of firms are the same mid-size auditor firm, and 0 if auditors in a pair are two different mid-size auditors. The test variable *Same_Big4* in Panels C and D is coded 1 if both auditors in a pair of firms are the same Big 4 firm, and 0 if auditors in a pair are the same non-Big 4 auditor. Control variables are not reported for brevity.

IX. ECONOMIC IMPORTANCE

In this section we calculate the economic significance of auditor style effects on the comparability of earnings. In the accruals-difference test reported in Table 2, the coefficient is 0.001 on *Same_Big4* that means that the average effect of auditor style on reported earnings is approximately 0.1 percent of total assets. However, the analysis of auditor changes in Table 3, Panel A, which is arguably a stronger test, suggests a larger magnitude closer to 0.4 percent of assets. The median firm in our study has an operating ROA (operating income scaled by average total assets) of 7.33 percent. Therefore, the auditor style effect due to a 0.4 percent shift in accruals (scaled by assets) will cause a 5.5 percent change in operating ROA for the average firm in the sample ($0.4/7.33$). There would be a similar percentage effect on firms' profit margins because sales and total assets are nearly the same for the median firm in the sample. The median ratio of sales to assets is 1.005, and the median operating profit margin is 7.2 percent. So a 0.4 percent shift in accruals would cause a 5.6 percent change in operating margin ($0.4/7.2$). In both ROA and profit margin calculations, a fraction of one percentage point in accruals (scaled by assets) has a nontrivial effect.

In the auditor fixed effect model, when total accruals is the dependent variable, the individual audit firm coefficients range from a low of -0.007 to a high of $+0.044$. All but one coefficient is significant at the $p < 0.10$ level (two-tailed), with an equal number of positive and negative coefficients. The coefficients are interpretable as percentage effects on total accruals, so the percentage range is from -0.7 to $+4.4$ percent of accruals, scaled by assets. Pairwise differences in audit firms' coefficients have an average absolute value of 0.026, or 2.6 percent accruals. As demonstrated in the ROA and profit margin analysis above, a percentage point change in accruals, scaled by assets, has a nontrivial effect on financial statements and financial ratios.

In the earnings covariation test in Table 4, the coefficient is 0.002 for *Same_Big4*. This implies that auditor style increases the covariance of earnings, on average, by 2.13 percent from a base of 0.094 ($0.002/0.094$). The effect is more difficult to interpret because it is a change in R^2 , but the percentage magnitude is in line with the above tests.

Because this is the first study to examine auditor style, we have no empirical evidence to inform our priors as to what the magnitudes should be. However, we believe the magnitudes for both the accrual and earnings covariation metrics are plausible and can be categorized as large enough to matter in an economic as well as statistical sense.

X. CONCLUSION

We expect firm-pairs in the same industry-year will have a similar accruals and earnings structure after controlling for firm-specific factors and common exogenous shocks. Our tests show that this is more likely to be the case if the firm-pair is audited by the same Big 4 accounting firm, which is evidence of an audit firm style effect in making accruals/earnings similar within an auditor's clientele. A single set of uniform accounting standards is often advocated as a means to increase comparability of financial statements, reflecting the rationale for the FASB-IASB convergence project. Our study documents that the role of an economic agent, the auditor, is also important in facilitating the production of accounting comparability. We argue that the Big 4 style effect arises from each audit firm having its own unique set of in-house rules with respect to the interpretation and implementation of GAAS (auditing standards) and the interpretation and enforcement of GAAP (accounting standards).

Our results have a number of implications. First, they provide support for Kothari et al. (2010) who conjecture that when standards are principles-based, economic agents such as auditors will develop their own in-house rules that give rise to comparability in the production of financial statements. We find support for the idea that auditors develop in-house rules to facilitate

comparability within their clientele. These auditor style effects also appear to reduce comparability between clients audited by different auditors, although we do not know if these effects are greater than what would occur in the absence of auditor style. We also contribute to the audit literature by showing that Big 4 accounting firms may have an effect on another earnings attribute that has not previously been investigated, namely, accounting comparability. We find that firms audited by Big 4 auditors have greater accounting comparability than firms audited by non-Big 4 auditors, which suggests another dimension in which the two auditor groups differ. It is also the case that each Big 4 audit firm has its own “style,” which affects accounting comparability and is therefore another source of variation within the Big 4 group of auditors.

While the purpose of our investigation is not to examine whether the accounting comparability arising from audit style improves earnings quality, our findings suggest this is a useful area for future research. On one hand, the in-house working rules of audit firms are beneficial if they minimize random errors by audit staff and random errors and/or intentional biases by clients, which would improve earnings quality within an auditor’s clientele. However, this effect also allows for the possibility of systematic differences between auditor clienteles. Our study reports some suggestive evidence that both effects occur. For example, Table 2 shows that firm-pairs in the same industry-year have smaller differences in abnormal accruals when audited by the same Big 4 auditor, and we know that larger abnormal accruals are associated with the likelihood of client misreporting (Dechow, Sloan, and Sweeney 1996; Dechow, Ge, Larson, and Sloan 2011). We also document in Table 1, Panel B that firm-pairs with smaller accrual-differences have more similar earnings time-series (*ECOMP_COV*), and Table 4 shows that stock returns of firm-pairs are more similar (*RET_COV*) when their earnings time-series are more similar. Together these results suggest that the accounting comparability, as defined in this study, leads to a more similar mapping of earnings to stock returns for firm-pairs and, therefore, is potentially value-relevant. However, an important alternative possibility is that “audit style” imposes consistency and uniformity at the expense of relevance and true comparability. As FASB (2010, para. Q23) cautions, uniformity may make things look the same, but uniformity is not comparability, where the objective is to make “like things” look alike, and “different things” look different. Therefore, a logical next step is to study the implications of auditor style on the quality and informativeness of earnings, and to examine if auditor-induced accounting comparability improves earnings quality or if it leads to uniformity at the expense of more informative accounting.

REFERENCES

- Acito, A. A., J. J. Burks, and W. B. Johnson. 2009. Materiality decisions and the correction of accounting errors. *The Accounting Review* 84 (3): 659–688.
- Ball, R., A. Robin, and J. S. Wu. 2003. Incentives versus standards: Properties of accounting income in four East Asian countries. *Journal of Accounting and Economics* 36 (1–3): 235–270.
- Bamber, L. S., J. Jiang, and I. Y. Wang. 2010. What’s my style? The influence of top managers on voluntary corporate financial disclosure. *The Accounting Review* 85 (4): 1131–1162.
- Banker, R. D., H. Chang, and Y. C. Kao. 2002. Impact of information technology on public accounting firm productivity. *Journal of Information Systems* 16 (2): 209–222.
- Barberis, N., and A. Shleifer. 2003. Style investing. *Journal of Financial Economics* 68 (2): 161–199.
- Barth, M. E., W. R. Landsman, M. Lang, and C. Williams. 2012. Are IFRS-based and US GAAP-based accounting amounts comparable? *Journal of Accounting and Economics* 54 (1): 68–93.
- Basu, S. 1997. The conservatism principle and the asymmetric timeliness of earnings. *Journal of Accounting and Economics* 24 (1): 3–37.
- Becker, C. L., M. L. DeFond, J. Jiambalvo, and K. R. Subramanyam. 1998. The effect of audit quality on earnings management. *Contemporary Accounting Research* 15 (1): 1–24.

- Bell, T., F. Marrs, I. Solomon, and H. Thomas. 1997. *Auditing Organizations Through a Strategic-Systems Lens: the KPMG Business Measurement Process*. Montvale, NJ: KPMG.
- Berger, P., and R. Hann. 2007. Segment profitability and the proprietary and agency costs of disclosure. *The Accounting Review* 82 (4): 869–906.
- Bertrand, M., and A. Schoar. 2003. Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics* 118 (4): 1169–1208.
- Blacconiere, W., J. Frederickson, M. Johnson, and M. Lewis. 2011. Are voluntary disclosures that disavow the reliability of mandated fair value information informative or opportunistic? *Journal of Accounting and Economics* 52 (2–3): 235–251.
- Boone, J., I. K. Khurana, and K. Raman. 2010. Do the Big 4 and the second-tier firms provide audits of similar quality? *Journal of Accounting and Public Policy* 29 (4): 330–52.
- Botosan, J., and M. Stanford. 2005. Managers' motives to withhold segment disclosures and the effect of SFAS No. 131 on analysts' information environment. *The Accounting Review* 80: 751–771.
- Bradshaw, M. T., and G. S. Miller. 2007. Will harmonizing accounting standards really harmonize accounting? Evidence from non-US firms adopting US GAAP. *Journal of Accounting, Auditing and Finance* 23 (2): 233–263.
- Bradshaw, M. T., G. S. Miller, and S. J. Serafeim. 2011. *Accounting Method Heterogeneity and Analysts' Forecasts*. Working paper, Boston College, University of Michigan, and Harvard University.
- Brown, L., and A. Pinello. 2007. To what extent does the financial reporting process curb earnings surprise games? *Journal of Accounting Research* 45 (5): 947–981.
- Burgstahler, D., and I. Dichev. 1997. Earnings management to avoid earnings decreases and losses. *Journal of Accounting and Economics* 24 (1): 99–126.
- Cassell, C., G. Giroux, L. Meyers, and T. Omer. 2013. The emergence of second-tier auditors in the US: Evidence from investor perceptions of financial reporting credibility. *Journal of Business Finance and Accounting* 40 (3–4): 350–372.
- Chow, C., and S. Rice. 1982. Qualified audit opinions and auditor switching. *The Accounting Review* 57 (2): 326–335.
- Comprix, J., and K. Muller. 2006. Asymmetric treatment of reported pension expense and income amounts in CEO cash compensation calculations. *Journal of Accounting and Economics* 42 (3): 385–416.
- Cushing, B., and J. Loebbecke. 1986. *Comparison of Audit Methodologies of Large Accounting Firms*. Sarasota, FL: American Accounting Association.
- Dechow, P., R. Sloan, and A. Sweeney. 1996. Causes and consequences of earnings manipulation: An analysis of firms subject to enforcement actions by the SEC. *Contemporary Accounting Research* 13 (1): 1–36.
- Dechow, P., W. Ge, C. Larson, and R. Sloan. 2011. Predicting material accounting misstatements. *Contemporary Accounting Research* 28 (1): 17–82.
- Dechow, P. M., and I. Dichev. 2002. The quality of accounting and earnings: The role of accrual estimation errors. *The Accounting Review* 77 (Supplement): 35–59.
- DeFond, M. L. 1992. The association between changes in client agency costs and auditor switching. *Auditing: A Journal of Practice & Theory* 11:16–31.
- DeFond, M., and K. R. Subramanyam. 1998. Auditor changes and discretionary accruals. *Journal of Accounting and Economics* 25 (1): 35–67.
- DeFond, M., X. Hu, M. Hung, and S. Li. 2011. The impact of mandatory IFRS adoption on foreign mutual fund ownership: The role of comparability. *Journal of Accounting and Economics* 51 (3): 240–258.
- De Franco, G., S. P. Kothari, and R. Verdi. 2011. The benefits of financial statement comparability. *Journal of Accounting Research* 49 (4): 895–931.
- Dichev, I. J., J. R. Graham, C. R. Harvey, and S. Rajgopal. 2013. Earnings quality: Evidence from the field. *Journal of Accounting and Economics* 56 (2–3, Supplement 1): 1–33.
- Dowling, C. 2009. Appropriate audit support system use: The influence of auditor, audit team, and firm factors. *The Accounting Review* 84 (3): 771–810.
- Doyle, J., W. Ge, and S. McVay. 2007. Accruals quality and internal control over financial reporting. *The Accounting Review* 82 (5): 1141–1170.

- Ernst & Young (E&Y). 1995. *Financial Reporting Developments: Accounting and Disclosure of Stock-Based Compensation Under SFAS 123*. New York, NY: E&Y.
- Financial Accounting Standards Board (FASB). 1976. *Accounting for Leases*. Statement of Financial Accounting Standards No. 13. Norwalk, CT: FASB.
- Financial Accounting Standards Board (FASB). 1980. *Qualitative Characteristics of Accounting Information*. Statement of Financial Accounting Concepts No. 2. Norwalk, CT: FASB.
- Financial Accounting Standards Board (FASB). 1988. *Accounting for Leases*. Statement of Financial Accounting Standards No. 98. Norwalk, CT: FASB.
- Financial Accounting Standards Board (FASB). 2010. *Conceptual Framework for Financial Reporting*. Statement of Financial Accounting Concepts No. 8. Norwalk, CT: FASB.
- Francis, J., L. E. Maydew, and H. C. Sparks. 1999. The role of Big 6 auditors in the credible reporting of accruals. *Auditing: A Journal of Practice & Theory* 18 (2): 17–34.
- Francis, J. 2004. What do we know about audit quality? *The British Accounting Review* 34 (4): 345–368.
- Francis, J., S. Huang, and I. Khurana. 2009. Does corporate transparency contribute to efficient resource allocation? *Journal of Accounting Research* 47 (4): 943–989.
- Francis, J. 2011. A framework for understanding and researching audit quality. *Auditing: A Journal of Practice & Theory* 30 (2): 125–152.
- Francis, J. R., and E. R. Wilson. 1988. Auditor changes: A joint test of theories relating to agency costs and auditor differentiation. *The Accounting Review* 63 (4): 663–682.
- Frankel, R., M. Johnson, and K. Nelson. 2002. The relation between auditors' fees for nonaudit services and earnings management. *The Accounting Review* 77 (Supplement): 71–105.
- Ge, W., D. Matsumoto, and J. Zhang. 2011. Do CFOs have style? An empirical investigation of the effect of individual CFOs on accounting practices. *Contemporary Accounting Research* 28 (4): 1141–1170.
- Greene, W. 2011. *Econometric Analysis*. 7th edition. Englewood Cliffs, NJ: Prentice Hall.
- Harris, M. 1998. The association between competition and managers' business segment reporting decisions. *Journal of Accounting Research* 36 (1): 111–128.
- Hay, D., W. Knechel, and N. Wong. 2006. Audit fees: A meta-analysis of the effect of supply and demand attributes. *Contemporary Accounting Research* 23 (1): 141–192.
- Johnson, V., I. Khurana, and J. K. Reynolds. 2002. Audit-firm tenure and the quality of financial reports. *Contemporary Accounting Research* 19 (4): 637–660.
- Johnson, W. B., and T. Lys. 1990. The market for audit services. *Journal of Accounting and Economics* 12 (1-3): 281–308.
- Jones, J. J. 1991. Earnings management during import relief investigations. *Journal of Accounting Research* 29 (2): 193–228.
- Joos, P., and M. Lang. 1994. The effects of accounting diversity: Evidence from the European Union. *Journal of Accounting Research* 32 (Supplement): 141–168.
- Kaplan, S., D. Williams, and K. Menon. 1990. The effects of audit structure on the audit market. *Journal of Accounting and Public Policy* 9 (3): 197–216.
- Khan, M., and R. Watts. 2009. Estimation and empirical properties of a firm-year measure of accounting conservatism. *Journal of Accounting and Economics* 48 (2-3): 132–150.
- Kinney, W. 1986. Audit technology and preferences for auditing standards. *Journal of Accounting and Economics* 8 (1): 73–89.
- Kothari, S., A. Leone, and C. Wasley. 2005. Performance matched discretionary accruals. *Journal of Accounting and Economics* 39 (1): 163–197.
- Kothari, S. P., K. Ramanna, and D. Skinner. 2010. Implications for GAAP from an analysis of positive research in accounting. *Journal of Accounting and Economics* 50 (2-3): 246–286.
- Krishnan, G. 2005. Did Houston clients of Arthur Andersen recognize publicly available bad news in a timely fashion? *Contemporary Accounting Research* 22 (1): 165–193.
- Land, J., and M. Lang. 2002. Empirical evidence on the evolution of international earnings. *The Accounting Review* 77 (Supplement): 115–133.

- Lang, M., M. Maffett, and E. Owens. 2010. *Earnings Comovement and Accounting Comparability: the Effects of Mandatory IFRS Adoption*. Working paper, The University of North Carolina, The University of Chicago, and University of Rochester.
- Lennox, C. 2000. Do companies engage in successful opinion shopping? *Journal of Accounting and Economics* 29: 321–337.
- Leuz, C., D. Nanda, and P. Wysocki. 2003. Earnings management and investor protection: An international comparison. *Journal of Financial Economics* 69 (3): 505–527.
- Menon, K., and D. Williams. 2004. Former audit partners and abnormal accruals. *The Accounting Review* 79 (4): 1095–1118.
- Petersen, M. A. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies* 22 (1): 435–480.
- Phillips, F., R. Libby, and P. Libby. 2013. *Fundamentals of Financial Accounting*. 4th edition. New York, NY: McGraw-Hill Irwin.
- Picconi, M. 2006. The perils of pensions: Does pension accounting lead investors and analysts astray? *The Accounting Review* 81 (4): 925–955.
- Reichelt, K., and D. Wang. 2010. National and office-specific measures of auditor industry expertise and effects on audit quality. *Journal of Accounting Research* 48 (3): 647–686.
- Revsine, L., D. Collins, W. B. Johnson, and H. F. Mittelstaedt. 2011. *Financial Reporting and Analysis*. 5th edition. New York, NY: McGraw-Hill Irwin.
- Reynolds, J. K., and J. Francis. 2000. Does size matter? The influence of large clients on office-level auditor reporting decisions. *Journal of Accounting and Economics* 30 (3): 375–400.
- Shu, S. 2000. Auditor resignations: Clientele effects and legal liability. *Journal of Accounting and Economics* 29 (2): 173–205.
- Smith, D. B. 1986. Auditor subject to opinions, disclaimers, and auditor changes. *Auditing: A Journal of Practice & Theory* 6 (Fall): 95–108.
- Teoh, S., and T. Wong. 1993. Perceived auditor quality and the earnings response coefficient. *The Accounting Review* 68 (2): 346–366.
- Vera-Muñoz, S., J. Ho, and C. Chow. 2006. Enhancing knowledge sharing in public accounting firms. *Accounting Horizons* 20 (2): 133–155.
- Zemi, M. 2012. Audit partner specialization and audit fees: Some evidence from Sweden. *Contemporary Accounting Research* 29 (1): 312–340.

APPENDIX A

Variable Definitions

Dependent Variables (all measures are in period t unless noted otherwise)

Difference in total accruals
(*Diff_Total_Accruals*) Equals the absolute value of the difference between total accruals of firm i and total accruals of firm j in firm-pair in year t . Total accruals are calculated as the difference between income before extraordinary items and cash flows from operations adjusted for cash flows from extraordinary items ($IB - (OANCF - XIDOC)$), scaled by beginning of year total assets. This variable measures how close total accruals are for two firms.

Difference in signed abnormal accruals (*Abn_Accr_Diff*) Equals the absolute value of the difference between abnormal accruals of firm i and abnormal accruals of firm j in firm-pair in year t . Abnormal accruals are calculated using Jones (1991) model of discretionary accruals as modified by Kothari et al. (2003). This variable measures how close abnormal accruals are for two firms.

ECOMP_COV Within-industry earnings co-movement in firm-pair firm i and firm j across 16 consecutive quarters, calculated as defined in Section III.

Explanatory Variables

Same_Big4 Coded 1 if both auditors in firm-pair firm i and firm j are the same Big 4 firm, 0 if auditors in firm-pair firm i and firm j are two different Big 4 auditors.

Control Variables

Accruals_Min Minimum value of total (or abnormal) accruals in firm-pair firm i and firm j .

Size_Diff Absolute value of difference in size in firm-pair firm i and firm j . Size equals natural logarithm of total assets.

Size_Min Minimum value of size in firm-pair firm i and firm j .

LEV_Diff Absolute value of the difference in leverage in firm-pair firm i and firm j , where leverage is a debt-to-assets ratio of a company.

LEV_Min Minimum value of leverage in firm-pair firm i and firm j .

MB_Diff Absolute value of difference in market-to-book ratio in firm-pair firm i and firm j . Market-to-book ratio is calculated as market value of equity divided by book value of equity.

MB_Min Minimum value of market-to-book ratio in firm-pair firm i and firm j .

CFO_Diff Absolute value of difference in cash flows from operations (scaled by total assets in year $t-1$) in firm-pair firm i and firm j .

CFO_Min Minimum value of scaled cash flows from operations in firm-pair firm i and firm j .

LossProb_Diff Absolute value of the difference in loss probability in firm-pair firm i and firm j . Loss probability is the proportion of quarters for which the firm reports a negative quarterly income before extraordinary items in the past 16 quarters.

(continued on next page)

APPENDIX A (continued)

<i>LossProb_Min</i>	Minimum value of loss probability in firm-pair firm <i>i</i> and firm <i>j</i> .
<i>STD_Sales_Diff</i>	Absolute value of the difference in standard deviation of quarterly sales in firm-pair firm <i>i</i> and firm <i>j</i> . Standard deviation of sales is calculated over the preceding 16 quarters.
<i>STD_Sales_Min</i>	Minimum value of standard deviation of quarterly sales in firm-pair firm <i>i</i> and firm <i>j</i> .
<i>STD_CFO_Diff</i>	Absolute value of the difference in standard deviation of quarterly operating cash flows in firm-pair firm <i>i</i> and firm <i>j</i> , where standard deviation of cash flows from operations is calculated over the preceding 16 quarters.
<i>STD_CFO_Min</i>	Minimum value of the standard deviation of quarterly cash flows from operations in firm-pair firm <i>i</i> and firm <i>j</i> .
<i>STD_Sales_Grth_Diff</i>	Absolute value of the difference in standard deviation of quarterly sales growth in firm-pair firm <i>i</i> and firm <i>j</i> , where standard deviation of sales growth is calculated over the preceding 16 quarters. Sales growth equals sales in current year <i>t</i> minus sales in year <i>t</i> –1 divided by sales in year <i>t</i> –1.
<i>STD_Sales_Grth_Min</i>	Minimum value of the standard deviation of quarterly sales growth in firm-pair firm <i>i</i> and firm <i>j</i> .
<i>STD_CFO_Diff</i>	Absolute value of the difference in standard deviation of quarterly operating cash flows in firm-pair firm <i>i</i> and firm <i>j</i> , where standard deviation of cash flows from operations is calculated over the preceding 16 quarters.
<i>STD_CFO_Min</i>	Minimum value of the standard deviation of quarterly cash flows from operations in firm-pair firm <i>i</i> and firm <i>j</i> .
<i>CFO_COMP_COV</i>	Within-industry cash flow co-movement across 16 consecutive quarters in firm-pair firm <i>i</i> and firm <i>j</i> , calculated as defined in Section III.
<i>RET_COV</i>	Within-industry return co-movement across 16 consecutive quarters in firm-pair firm <i>i</i> and firm <i>j</i> , calculated as defined in Section III.

Are Seemingly Self-Serving Attributions in Earnings Press Releases Plausible? Empirical Evidence

Michael D. Kimbrough
University of Maryland

Isabel Yanyan Wang
Michigan State University

ABSTRACT: Seemingly self-serving attributions either attribute favorable performance to internal causes (enhancing attributions) or poor performance to external causes (defensive attributions). Managers presumably provide such attributions in earnings press releases to heighten (dampen) investors' perceptions of the persistence of good (bad) earnings news, thereby increasing (decreasing) the market reward (penalty) for good (bad) earnings news. Building on attribution theory and prior research on earnings commonality, this study investigates cross-sectional differences in investors' responses to quarterly earnings press releases that contain seemingly self-serving attributions. Using a random sample of press releases from 1999 to 2005, we find that firms that provide defensive attributions to explain earnings disappointments experience less severe market penalties when: (1) more of the their industry peers also release bad news, and (2) their earnings share higher commonality with industry- and market-level earnings. On the other hand, firms that provide enhancing attributions to explain good earnings news reap greater market rewards when: (1) more of their industry peers release bad news, and (2) their earnings share lower commonality with industry- and market-level earnings. Collectively, our results demonstrate that investors neither ignore seemingly self-serving attributions nor accept them at face value, but rely on industry- and firm-specific information to assess their plausibility.

Keywords: *narrative disclosure; attribution; plausibility.*

Data Availability: *Data are publicly available from the sources identified in the text.*

We are grateful for constructive comments from Linda Bamber, Dan Collins, John Harry Evans III (senior editor), Mary Margaret Frank, Richard Frankel, John Hassell, John (Xuefeng) Jiang, Joan Luft, Bin Ke (editor), Molly Mercer, Kathy Petroni, K. Ramesh, Mike Shields, two anonymous reviewers, brownbag participants at Michigan State University, workshop participants at American University; Arizona State University; The University of Georgia; University of Pennsylvania; The Pennsylvania State University; University of South Florida; Binghamton University, SUNY; Buffalo State College, SUNY; The University of Tennessee–Knoxville; University of Virginia Darden School of Business; Washington University at St. Louis; and participants at the 2009 FARS Midyear Meeting for insightful comments that helped us improve this paper. We also thank Myron Chang, John Thelon, and Shawn Xu for excellent research assistance.

Editor's note: Accepted by Bin Ke.

Submitted: September 2011
Accepted: September 2013
Published Online: October 2013

I. INTRODUCTION

Managers often provide causal explanations for earnings news by linking earnings performance to internal actions or to external causes.¹ These “attributions” potentially help investors assess the implications of current earnings news, such as earnings persistence. A well-documented pattern from prior research on attributions (Ingram and Frazier 1983; Staw, McKechnie, and Puffer 1983; Salancik and Meindl 1984; Baginski, Hassell, and Hillison 2000) is that managers are more likely to attribute good news to internal causes (enhancing attributions) and bad news to external causes (defensive attributions). Managers presumably provide such seemingly self-serving attributions to heighten (dampen) investors’ perceptions of the persistence of good (bad) earnings news, thereby increasing (decreasing) the market reward (penalty) for good (bad) earnings news.

Investors face a challenge when interpreting seemingly self-serving attributions because they may provide unbiased information about future earnings or they may reflect managers’ psychological biases or opportunism (Merkl-Davies and Brennan 2007). In this study, we examine the information that investors appear to rely upon when assessing the plausibility of seemingly self-serving attributions in earnings press releases. Specifically, we test whether the market’s reactions to earnings announcements that contain seeming self-serving attributions vary with: (1) the concurrent performance of other firms in the same industry, and (2) the commonality of the firm’s earnings with the market and its industry, defined as the historical degree of co-movement between a firm’s earnings and market- and industry-level earnings.

Our study addresses two important issues that arise from prior literature. First, do investors calibrate their responses to earnings announcements that contain seemingly self-serving attributions? Baginski et al. (2000) and Baginski, Hassell, and Kimbrough (2004) find that the presence of attributions is associated with heightened stock price reactions to management forecasts, suggesting that the attributions “enhance *either* the precision *or* credibility of the management forecasts” (Baginski et al. 2000, 371; emphasis added). However, neither study focuses on the subset of seemingly self-serving attributions. We believe that seemingly self-serving attributions are of particular interest because they pose challenges for investors in distinguishing between those that are truly informative versus those that are driven by managers’ psychological biases or opportunism. It is not obvious how investors respond to this challenge because investors may ignore seemingly self-serving attributions or accept them at face value. A third possibility we examine is that investors use other relevant information to assess the plausibility of seemingly self-serving attributions.

Second, how do investors assess the plausibility of seemingly self-serving attributions in unstructured real-world settings? Barton and Mercer (2005) provide persuasive experimental evidence that the plausibility of managers’ defensive attributions affects analysts’ responses to those attributions. Because Barton and Mercer’s (2005) experimental materials allowed the analyst participants to easily detect implausible attributions, it remains an open question as to *how* capital market participants assess the plausibility of seemingly self-serving attributions in actual market settings in which assessing plausibility is more complex for several reasons. First, managers are reluctant to make self-serving attributions that can be easily refuted (Snyder, Stephan, and Rosenfeld 1978), suggesting that implausible attributions may not be easy to identify. Second, the causes that management cites—particularly the internal actions cited to explain good earnings

¹ In 2001, the FASB’s Steering Committee Report titled, *Improving Business Reports: Insight Into Enhancing Voluntary Disclosures*, describes voluntary disclosure practices in various industry groups. Attributions, both internal and external, existed in each of the eight industries studied (Financial Accounting Standards Board [FASB] 2001).

news—are often not verifiable due to their lack of observability to investors. Finally, even when the cited causes themselves are observable, the validity of the causal *links* that management draws between the cited causes and earnings performance are often not verifiable to investors.²

We measure the plausibility of seemingly self-serving attributions as the extent to which market reactions to earnings announcements that contain such attributions align with managers' presumed intent, which is to increase (decrease) the market reward (penalty) for good (bad) earnings news. Motivated by attribution theories, we first focus on concurrent industry performance as a potential plausibility measure. Drawing on Kelley's (1971) framework in attribution theory, Bettman and Weitz (1983) argue that both enhancing and defensive attributions are more believable when the performance of the relevant comparison group is bad. Therefore, we use concurrent industry performance to capture how the relevant comparison group performs and expect seemingly self-serving attributions to be more plausible when more industry peers perform poorly.

Our second plausibility measure is earnings commonality. A firm's earnings reflects not only firm-specific actions, but also common shocks in the industry and the market (Brown and Ball 1967; Magee 1974; Foster 1978, 1981; Gong, Li, and Zhou 2013). Earnings commonality is a useful gauge of the degree of control that firms have over their economic environments because it reveals the extent to which external market and/or industry forces, as opposed to firm-specific factors such as managerial actions, drive a firm's earnings performance. Higher earnings commonality indicates that the firm has less control over its economic environment and *vice versa*. So we predict that investors will view defensive attributions, in which managers claim less control over their firms' outcomes, as more plausible when earnings commonality is higher, and view enhancing attributions, in which managers assert greater control over their firms' outcomes, as more plausible when earnings commonality is lower.

Using a sample of earnings press releases from 94 randomly selected firms from 1999 to 2005, we document cross-sectional differences in the market's response to earnings announcements that contain either defensive or enhancing attributions that are consistent with our predictions. Specifically, we find that firms that provide defensive attributions to explain earnings disappointments experience less severe market penalties when: (1) more of their industry peers also release bad news, and (2) their earnings share higher commonality with industry- and market-level earnings. On the other hand, firms that provide enhancing attributions to explain good earnings news reap greater market rewards when: (1) more of their industry peers release bad news, and (2) their earnings share lower commonality with industry- and market-level earnings. Collectively, our results suggest that investors neither completely ignore seemingly self-serving attributions nor accept them at face value, but use industry- and firm-specific information to assess their plausibility. Further analyses reveal that investors' use of industry peer performance and earnings commonality information appears justified because investors' perceptions are consistent with the association between the plausibility measures and the *ex post actual* persistence of earnings surprises.

We also conduct supplemental analyses that compare the impacts of the plausibility factors for our main sample to their impacts for a sample of earnings announcements that do not contain seemingly self-serving attributions. Because managers may choose whether to provide a seemingly self-serving attribution, we develop a comprehensive model of the decision to provide seemingly self-serving attributions to address the potential endogeneity that might arise when comparing across the two samples. After controlling for endogeneity, our results suggest that the plausibility factors play a unique role in affecting investors' reactions to seemingly self-serving attributions.

² For example, while declines in macroeconomic indicators may be observable and verifiable (e.g., GDP), the validity of management claims that macroeconomic factors rather than bad management were responsible for poor financial performance may not be directly observable and verifiable.

Our study makes several contributions to the literature. First, our study complements Barton and Mercer (2005) by providing additional insight into the effect of plausibility on markets’ reactions to earnings announcements that contain *enhancing* attributions, while their experiment focuses only on *defensive* attributions. More importantly, we extend Barton and Mercer (2005) by providing empirical evidence on the information that investors use to assess seemingly self-serving attributions in unstructured real-world settings in which the plausibility of such attributions is typically not obvious. Understanding the circumstances under which investors may view a seemingly self-serving attribution more skeptically can help managers decide when to provide supporting details to make their narrative disclosures more believable.³

Second, our study adds to the archival evidence from Baginski et al. (2000) and Baginski et al. (2004) on the informativeness of attributions. Neither study separately examines seemingly self-serving attributions. Investors’ responses to seemingly self-serving attributions in earnings press releases are particularly interesting because they have implications for firms’ valuations (Barton and Mercer 2005) and pose unique interpretation challenges for investors given the different motivations behind such attributions.

Finally, our study answers the call by Koonce and Mercer (2005) who argue that archival researchers can gain additional insights by appealing to psychology theories. Our study also answers the call by Koonce, Seybert, and Smith (2011) for archival research focusing on how investors evaluate the *content*, rather than simply the amount, of disclosures. By analyzing the detailed content of earnings press releases and drawing on attribution theory, our study provides insight into the cues investors use to interpret narrative disclosures like seemingly self-serving attributions that could be either truthful or misleading.

Section II next discusses related research and develops hypotheses. Section III describes the sample and the characteristics of attributions. Section IV discusses our research design. Section V describes the results, and Section VI concludes.

II. RELATED RESEARCH AND HYPOTHESIS DEVELOPMENT

Managerial Motivations for and Perceived Informativeness of Attributions

Despite the pervasiveness of seemingly self-serving attributions in various types of narrative disclosures,⁴ there is little consensus as to why these attributions exist. Such attributions could truthfully reveal managers’ private information. Alternatively, they may reflect managers’ flawed, albeit genuine, beliefs. For instance, Staw (1980) argues that the tendency to attribute bad outcomes to external causes arises from high ego-defensiveness, which drives one to search for explanations that relieve oneself of responsibility. Miller and Ross (1975) argue that individuals make plans that they believe will lead to success. When favorable outcomes occur, individuals therefore attribute

³ Mercer (2005) argues that including supporting details can enhance the plausibility of a management disclosure by increasing its verifiability. In addition, Hutton, Miller, and Skinner (2003) show that investors respond more readily to verifiable rather than soft talk disclosures while Merkley, Bamber, and Christensen (2013) show that analysts find management forecasts to be more credible when managers provide supplemental forecasts of income statement components in addition to the bottom-line forecast.

⁴ Such narratives appear in the letter to shareholders (Abrahamson and Amir 1996; Bettman and Weitz 1983; Staw et al. 1983; Smith and Taffler 1992, 2000), management’s discussion and analysis (MD&A) included in annual reports (Ingram and Frazier 1983; Frazier, Ingram, and Tennyson 1984; Tennyson, Ingram, and Dugan 1990; Bryan 1997; Clarkson, Kao, and Richardson 1999), the chairman’s report (Clatworthy and Jones 2003), and in supplemental discussions included with earnings-related news releases, such as earnings press releases and voluntary management earnings forecasts (Baginski et al. 2000; Hutton et al. 2003; Baginski et al. 2004; Davis, Piger, and Sedor 2012).

them to the success of their own plans. Because unfavorable outcomes are unexpected and unintended, individuals will seek external causes to explain them.

Departing from psychological explanations, Staw et al. (1983) provide evidence that the self-serving pattern in attributions reflects managers' attempts to influence investor impressions. Specifically, they find that defensive attributions in the form of external attributions of poor performance are more common after poor prior stock returns, while enhancing attributions in the form of internal attributions of good performance are more common after a positive prior stock price run-up. They interpret these findings as evidence that managers seek to use such attributions either to blunt investor disappointment or to maintain a positive perception among investors. They also find that enhancing attributions are associated with subsequent stock sales by executives, which they interpret as further evidence that enhancing attributions primarily reflect managers' calculated attempts to influence investor impressions rather than their genuine expressions of optimism arising from psychological causes.

Given the different motivations, it is not obvious whether investors view seemingly self-serving attributions as generally informative (Koonce et al. 2011). The weight investors place on seemingly self-serving attributions when assessing earnings persistence depends on their priors about the proportion of truthful managers in the population. At one extreme, concerns about managers' psychological biases, managerial opportunism, and the lack of verifiability of seemingly self-serving attributions may lead investors to assume no managers are truthful and, therefore, to place no weight on seemingly self-serving attributions. Consistent with this notion, Hutton et al. (2003) find that investors do not respond to soft talk disclosures that accompany management earnings forecasts, which include seemingly self-serving attributions. They attribute this result to the lack of verifiability of soft talk disclosures. At the other extreme, investors may naively assume that all managers are truthful and, therefore, place full weight on seemingly self-serving attributions. Consistent with this possibility, Staw et al. (1983) provide limited evidence that stock price performance improves following enhancing attributions. More generally, investors may partially rely on such attributions if their priors about the proportion of truthful managers fall between these two extremes.

While investors can rely solely on their priors about the distribution of manager types in the population to assess the average plausibility of seemingly self-serving attributions, they can potentially improve their assessment of the plausibility of individual seemingly self-serving attributions by augmenting their priors with other relevant information to revise their expectations, consistent with Bayesian updating. Therefore, we examine whether investor responses to earnings announcements accompanied by seemingly self-serving attributions vary based on information relevant to assessing the plausibility of such attributions, which one would expect if investors use this information to update their priors in Bayesian fashion.⁵

Related to the last possibility, Barton and Mercer (2005) provide *experimental* evidence that analysts' responses to defensive attributions vary with plausibility. Specifically, they examine how analysts respond to a firm's claim that its disappointing performance was due to a major earthquake

⁵ A number of studies in different contexts show that investors do not simply take disclosures at face value but use other information to determine their plausibility. Hirst, Koonce, and Simko (1995) provide experimental evidence that investors condition their responses to analyst reports on the perceived incentives of analysts to bias their reports. Koch (2002) and Frost (1997) provide empirical evidence consistent with Hirst et al. (1995) that investors condition their responses to management voluntary disclosures on whether the firms making the disclosure are financially distressed. Specifically, Koch (2002) shows that the market discounts good news management earnings forecasts issued by financially distressed U.S. firms, while Frost (1997) shows that the market discounts the positive-tone disclosures of distressed U.K. firms. Hodge, Hopkins, and Pratt (2006) provide experimental evidence that investors consider managers' incentives in assessing managers' classification choices when the classification choices are made in a discretionary regime but not when the classification choices are made in a mandated regime.

in Turkey. They manipulate the plausibility of this explanation by varying the distance between the firm's operations and the earthquake's epicenter. In the implausible (plausible) condition, this distance was greater (smaller). Participants in both conditions had easy access to a map that allowed them to verify the plausibility of management's explanation. Barton and Mercer (2005) find that if the explanation is implausible, then analysts punish the firm through lower earnings forecasts and a higher cost of capital. If the explanation is plausible, then analysts reward the firm through higher earnings forecasts but not a lower cost of capital.

Barton and Mercer's (2005) experimental evidence highlights the importance of plausibility in interpreting seemingly self-serving attributions. We provide additional insight into how investors assess seemingly self-serving attributions in contexts in which plausibility is harder to assess because (1) managers are unlikely to make self-serving attributions that are easily refuted (Snyder et al. 1978), (2) the causal factors that management cites may not be observable to investors, and/or (3) the purported causal links between the cited causes and firm performance may not be directly verifiable to investors.

Hypothesis Development

Managers presumably attribute good (bad) news to internal (external) causes to achieve greater rewards for good news earnings announcement (less severe penalties for bad news earnings announcement) by heightening (dampening) investors' perceptions of the persistence of good (bad) news. This assumption is consistent with Barton and Mercer (2005, 514) who argue:

Firm managers ... have incentives to provide disclosures that point to permanent factors when performance is good and to temporary factors when performance is poor. Managers also have incentives to attribute good performance to internal factors and poor performance to external factors ... Such explanations ... should increase analysts' and investors' beliefs about management's ability to generate positive abnormal returns. In sum, managers have incentives to provide self-serving disclosures that attribute good financial performance to permanent internal factors and poor performance to temporary external factors.

The internal/external dichotomy in attribution theory closely correlates to the concept of controllability (Bettman and Weitz 1983), which is relevant to assessing persistence because events over which management asserts control are presumably more replicable, while events over which management has less control are presumably less likely to recur. In our study, when managers attribute good performance to internal causes, they signal to investors that similar outcomes are likely to occur in the future under their control or stewardship, thereby magnifying the perceived persistence of positive earnings surprises. Conversely, when managers distance themselves from a negative earnings surprise through defensive attributions, they signal that similar outcomes are unlikely to occur in the future, thereby mitigating the perceived persistence of negative earnings surprises. To the extent external events are less likely to recur, investors would ascribe lower persistence to negative earnings surprises attributed to external events.⁶

⁶ As described in Appendix A, attributions that we classify as external include one-time events such as weather or catastrophe. We also classify macroeconomic events as external. The relevant consideration for our study is whether citing unfavorable macroeconomic indicators for a current earnings disappointment provides a reasonable basis for investors to infer that the current earnings disappointment is temporary. To provide insight into this question, we examined the time-series of quarterly GDP from January 1947 through October 2011 and monthly retail sales from January 1992 through March 2012. We found that 62 percent of quarterly GDP downturns lasted only one quarter, 19 percent lasted two quarters, and 15 percent lasted three quarters. In only one instance has a GDP decline lasted as long as four quarters. In addition, we found that 73 percent of monthly downturns in retail sales lasted only one month, and 24 percent lasted two months. Based on this historical evidence, it is reasonable for investors to infer that bad earnings news attributed to unfavorable macroeconomic circumstances is likely to be relatively short-lived.

To examine whether investors consult relevant information to determine the plausibility of the signals management conveyed through seemingly self-serving attributions, we build on both attribution theory and prior research on earnings and stock returns synchronicity to identify factors that might reflect the plausibility of seemingly self-serving attributions. Kelley's (1971) framework in attribution theory suggests that an observed outcome is the net result of internal and external causes that can either contribute to or hinder the outcome. Internal causes receive more (less) weight in explaining an observed outcome when the outcome occurs in the presence of external factors that hinder (contribute to) the outcome (Kelley 1971). Bettman and Weitz (1983) suggest that an indicator of the existence of external factors that hinder corporate performance is bad performance by the relevant comparison group. Hence, building on the Kelley (1971) framework, they argue that enhancing (defensive) attributions should be more believable when the performance of the relevant comparison group is bad because the presence of external factors that hinder performance should cause investors to increase (decrease) the weight on internal actions in explaining the firm's own good (bad) performance.

These predictions are consistent with broader attribution theories, which posit that one way to assess the plausibility of an attribution of individual performance either to external stimuli or to the individual is to refer to consensus information about how others responded to the same stimuli. Consensus information that indicates that others exposed to the same stimuli experienced similar (differing) performance outcomes makes it more plausible that the individual's performance is attributable to the stimuli (the individual). Koonce, Williamson, and Winchel (2010) provide experimental evidence verifying how investors use consensus information. They show that investors use information about the inaccuracy of similar firms' accounting estimates when assessing whether a firm's own inaccurate accounting estimates are due to external stimuli in the form of inherent uncertainty concerning the firm's operating environment or to management's own opportunistic behavior.

In the current context, attribution theory suggests that both defensive and enhancing attributions are more plausible when other firms facing similar economic circumstances perform poorly. We use same-industry peer firms as the relevant comparison group because industry peers face similar economic circumstances (Bhojraj, Lee, and Oler 2003) and analysts and investors react to earnings information of a firm's industry peers in updating their beliefs of the firm's future prospects (Lim, Ro, and Pyo 2001; Ramnath 2002). Based on the foregoing, we propose the following hypotheses, stated in the alternative form.

H1a: Conditional on providing a seemingly self-serving attribution in the earnings announcement, firms that announce a positive earnings surprise experience more favorable market reactions when more of their industry peers announce bad news prior to their earnings announcements.

H1b: Conditional on providing a seemingly self-serving attribution in the earnings announcement, firms that announce a negative earnings surprise experience less negative market reactions when more of their industry peers announce bad news prior to their earnings announcements.

We also identify earnings commonality as another that factor investors can use to assess the plausibility of seemingly self-serving attributions. The concept of earnings commonality or synchronicity stems from prior research that shows that a firm's earnings reflects not only firm-specific actions, but also common shocks in the industry and the market (Brown and Ball 1967; Gonedes 1973; Magee 1974; Foster 1978, 1981; Gong et al. 2013).⁷ Higher commonality

⁷ In this study, we use the word "synchronicity" interchangeably with "commonality" because both terms indicate how much firms' earnings or stock returns move together with that of the market and the industry.

indicates greater influence of market/industry factors and *lower* commonality implies greater influence from *firm-specific* actions. Supporting this interpretation of *low* commonality, Gong et al. (2013) show that firms with lower earnings commonality are more likely to provide management forecasts and investors react more strongly to such forecasts because they contain more firm-specific information.

Given that investors incorporate earnings commonality information in firm valuation (Magee 1974; Gong et al. 2013),⁸ it is possible that they use this information to assess the plausibility of seemingly self-serving attributions. To the extent that earnings commonality reflects the degree of control that a firm has over its economic environment, it is relevant to assessing seemingly self-serving attributions, in which managers either assert greater control over their firms' good outcomes or claim less control over their firms' bad outcomes. Consistent with this expectation, Tse and Tucker (2009, 903) state that "a necessary condition for managers to credibly assign blame for their firm's earnings shortfall to market or industry factors is the existence of some earnings synchronicity. Otherwise, investors would have no reason to believe that any of the earnings shortfall *is outside managers' control*" (emphasis added). We expect investors to view enhancing (defensive) attributions as more plausible if a firm has typically exhibited low (high) earnings commonality. Therefore, we propose the following hypotheses, stated in the alternative form.

H2a: Conditional on providing a seemingly self-serving attribution in the earnings announcement, firms that announce a positive earnings surprise experience more favorable market reactions when their earnings have lower commonality with industry- and market-level earnings.

H2b: Conditional on providing a seemingly self-serving attribution in the earnings announcement, firms that announce a negative earnings surprise experience less negative market reactions when their earnings have higher commonality with industry- and market-level earnings.

III. SAMPLE

Sample Selection

To economize on the costs of hand collection of data while maintaining the generalizability of our results, we draw a random sample of firms making quarterly earnings announcements during the period 1999–2005. Focusing on a small random sample allows detailed hand-collection on a representative sample at a reasonable cost and is common in disclosure research (e.g., Pownall, Wasley, and Waymire 1993; Skinner 1994; Hutton et al. 2003). We first identify all firms in the Compustat/CRSP merged quarterly file with non-missing quarterly earnings announcement dates and non-missing values for quarterly net income for all quarters in 1999. We retain observations with (1) consensus I/B/E/S forecasts for all quarters in 1999, and (2) stock returns surrounding all quarterly earnings announcement dates in 1999 on CRSP. These procedures result in a sample of 3,301 firms. We then randomly select 100 firms from this sample. When we divide the 3,301 firms into three equally sized groups based on market capitalization as of the end of year 1999, we find that our random sample consists of 30 firms from the small-size group, 38 from the medium-size

⁸ Investors can assess the degree of a firm's earnings commonality by monitoring how much the firm's earnings has historically co-moved with other firms in response to observable common shocks in the market and the industry such as monetary policy, interest rates, consumer demand, industry competition, and raw material prices (Brown and Ball 1967; Magee 1974).

group, and 32 from the large-size group. Thus, our sample appears representative of a variety of firm sizes.⁹

For our random sample of 100 firms, we obtain all available earnings announcement dates, net income amounts, and stock returns around earnings announcement dates for the 28 fiscal quarters from 1999 through 2005. We identify 2,142 firm-quarters of the 2,800 possible firm-quarters that meet our criteria. Our sample declines to 1,912 firm-quarters after eliminating observations for which we could not obtain the actual press releases either from Factiva, Lexis-Nexis, or the company website. We manually code these earnings press releases for attributions explaining the realized quarterly earnings performance. After eliminating observations with insufficient information on the various independent variables, our sample reduces to 1,790 firm-quarters related to 98 firms. For our hypothesis testing, we identify 1,023 firm-quarters related to 94 firms that provide a seemingly self-serving attribution, using procedures described below. Table 1 summarizes our sample selection procedures.¹⁰

Classification of Attributions

We read each earnings press release and logged every statement we identified that explained current period performance. For each statement that we identified, we coded (1) whether the cause being cited had a favorable or unfavorable impact on current earnings, and (2) whether the cause being cited was attributed to internal or external factors. We classified an earnings announcement press release as seemingly self-serving if the press release related to a positive (negative) earnings surprise and had at least one causal statement that linked favorable (unfavorable) factors to internal (external) causes.¹¹ Hence, our seemingly self-serving sample consists of press releases related to positive (negative) surprises with at least one enhancing (defensive) attribution. Appendix A describes our coding scheme and offers examples of seemingly self-serving attributions that we code.¹²

Besides seemingly self-serving attributions, we also find attributions that are incentive-incompatible or that cite factors that offset overall earnings news. For positive (negative) earnings surprises, an incentive-incompatible attribution cites a factor that affects earnings positively (negatively) but attributes the factor to *external* (*internal*) causes. An offsetting attribution cites a factor that affects earnings negatively (positively) in a press release that reports a positive (negative) earnings surprise. We group press releases that either contain no attribution or contain only incentive-incompatible or offsetting attributions into the non-self-serving sample.

Table 2 provides evidence on the pattern and intensity of attributions in both seemingly self-serving and non-self-serving samples. Panels A and B of Table 2 demonstrate that multiple types of attributions can exist in a single press release. For the seemingly self-serving sample, 17.67 percent

⁹ Our random sample spans 42 six-digit GICS codes. It appears that a few industries have slightly higher percentage of firms in our sample. Untabulated analysis shows that these industries also account for similar percentages in the Compustat population during the same time period.

¹⁰ Only four firms provided self-serving attributions in 100 percent of their press releases and only four firms never provided self-serving attributions in their press releases. So the seemingly self-serving and non-serving samples each contain 94 firms although we coded press releases for a total of 98 firms.

¹¹ The coding of attributions requires judgment. Both authors coded a pilot sample of 50 press releases and compared coding for consistency. For any differences in judgment, we discussed the rationale for our coding and reached a consensus. Each author then coded half of the remaining press releases. Finally, one of the authors checked the entire coding to ensure sample-wide consistency. Untabulated analysis indicates there are no statistically significant differences between the coding by each author. Because the coding was performed prior to gathering the remaining regression variables, such as market returns around earnings announcements, knowledge of the levels of the other variables did not influence our coding or lead to bias in favor of our findings.

¹² Untabulated analysis indicates that most factors attributed in defensive attributions can be viewed as common (shared) external forces such as consumer demand, economic conditions, material pricing, foreign exchange and interest rates, weather, and competition.

TABLE 1
Sample Selection and Variable Definitions

Panel A: Sample Selection

	Firms	Firm-Quarters
Firms listed on Compustat with non-missing quarterly earnings announcement dates and non-missing quarterly net income amounts for 1999	6,869	—
Less: firms with missing I/B/E/S consensus forecasts for any quarter in 1999	−2,517	—
Less: firms with missing stock returns on CRSP surrounding any quarterly earnings announcement in 1999	−1,051	—
Total Sample of Firms for Random Selection Procedures	3,301	—
Less: firms eliminated as a result of random selection procedures	−3,201	—
Random Sample for the period 1999–2005	100	2,800
Less: missing earnings announcement dates or net income amounts in Compustat or missing earnings announcement period returns on CRSP (other than year 1999)	—	−658
Less: missing earnings press releases	−1	−230
Less: observations with insufficient information to calculate earnings surprises	−1	−99
Less: observations with missing information for earnings response coefficient determinants	—	−23
Random Sample with required data	98	1,790
Subset without seemingly self-serving attributions	94	767
Subset with seemingly self-serving attributions (Main Sample)	94	1,023

Panel B: Variable Definitions

Variable	Definition
<i>AF</i>	number of analysts providing annual earnings forecasts for the firm in I/B/E/S;
<i>BadQtr</i>	percentage of previously announcing peer firms (in the same six-digit GICS code) that announce earnings disappointments prior to firm <i>i</i> 's quarterly earnings announcement. A minimum of five prior announcers is required;
<i>BTM</i>	ratio of the book value of equity to market value of equity at the beginning of the quarter;
<i>CAR</i>	three-day cumulative abnormal returns centered on the earnings announcement date, where abnormal returns are calculated using a market model. We estimate the market model parameters using a value-weighted market return over a 200-day period ending 31 trading days before the earnings announcement date. The abnormal return is the actual return on each day minus the expected return estimated by applying the market model parameters;
<i>Common</i>	R^2 from a regression performed over the 20 prior quarters of firm <i>i</i> 's quarterly ROA on an industry ROA index and a market ROA index over the same quarters. The industry ROA index is the average ROA of all other firms in the same six-digit GICS code and the market ROA index is the average ROA of all other non-industry firms in Compustat;
$\Delta Exchange rates$	change in the U.S. dollar exchange rate during the quarter;
<i>ForecastBias</i>	1 if managers issue an earnings forecast for the current quarter that turns out to be optimistic (i.e., greater than actual earnings), and 0 otherwise;

(continued on next page)

TABLE 1 (continued)

Variable	Definition
<i>FutureROA</i>	average quarterly ROA for the four quarters immediately following the quarter;
<i>%ΔGDP</i>	percentage seasonal change in GDP during the quarter;
<i>Hightech</i>	1 if a firm is from four-digit SIC codes 2833–2836 (drugs), 3570–3577 (computers), 3600–3674 (electronics), 7371–7379 (programming), 8731–8734 (R&D services), and 0 otherwise;
<i>ΔInflation</i>	change in the consumer price index during the quarter;
<i>ΔInterestrates</i>	change in interest rates during the quarter;
<i>IO%</i>	percentage of institutional ownership;
<i>IS_trade</i>	1 if there are insider trading activities during the 30 days before and 30 days after the earnings announcement, and 0 otherwise;
<i>LagROA</i>	lag of <i>FutureROA</i> ;
<i>Lambda</i>	Inverse Mills Ratio calculated according to the Heckman (1979) procedure based on the probit model in Equation (3);
<i>Large</i>	1 when the absolute value of earnings surprise is greater than 0.1, and 0 otherwise;
<i>Loss</i>	1 when actual earnings for the quarter are negative, and 0 otherwise;
<i>M&A</i>	1 if the firm completed a business combination during the previous four quarters, and 0 otherwise;
<i>NegEarningsChg</i>	1 if the seasonal change in quarterly earnings is negative, and 0 otherwise;
<i>Numsegments</i>	number of business segments for the firm;
<i>Plaus_BadQtr</i>	1 if <i>BadQtr</i> is at or above the sample median, and 0 otherwise;
<i>Plaus_Common</i>	1 for positive surprises (negative surprises) where <i>Common</i> is below (above) the sample median, and 0 otherwise;
<i>Positive</i>	1 if the earnings surprise for the quarter is greater than or equal to 0, and 0 otherwise;
<i>Qtr4</i>	1 if announced earnings pertain to the fourth fiscal quarter, and 0 otherwise;
<i>Regulated</i>	1 if a firm is from four-digit SIC codes 4842–4813 (telephone), 4833 (TV), 4841 (cable), 4811–4899 (communications), 4922–4924 (gas), 4931 (electricity), 4941 (water), 6021–6023, 6035–6036, 6141, 6311, 4321, 6331 (financial), and 0 otherwise;
<i>Restructure</i>	1 if the quarterly special item is less than or equal to −5 percent of total assets in any of the previous four quarters, and 0 otherwise;
<i>Retvol</i>	standard deviation of daily returns estimated over the 100-trading-day window that ends two days before the quarter’s earnings announcement;
<i>Selfserving</i>	1 if a firm provides a seemingly self-serving attribution in its earnings press release, and 0 otherwise;
<i>Size</i>	log of market value of equity as of the beginning of the quarter;
<i>SurprisePos</i>	earnings surprise when the surprise is greater than or equal to 0, and 0 otherwise. Earnings surprise is measured as actual earnings minus the latest mean consensus forecasts as reported in the I/B/E/S unadjusted files, scaled by the firm’s stock price two days prior to the earnings announcement date;
<i>SurpriseNeg</i>	the <i>absolute</i> value of earnings surprise when the surprise is less than 0, and 0 otherwise; and
<i>TimeTrend</i>	quarterly time trend where 1999Q1 = 1, 1999Q2 = 2, etc.

TABLE 2
Attribution Patterns in Earnings Press Releases and Descriptive Statistics

Panel A: Attribution Patterns for Positive Surprises

Attribution Pattern	Number (Percentage) of Press Releases with Indicated Pattern	
	Seemingly Self-Serving Sample	Non-Self-Serving Sample
Enhancing only	123 (17.67%)	
Enhancing and incentive-incompatible	102 (14.66%)	
Enhancing and offsetting	188 (27.01%)	
Enhancing, incentive-incompatible, and offsetting	283 (40.66%)	
Incentive-incompatible only	—	67 (12.96%)
Offsetting only	—	167 (32.30%)
Incentive-incompatible and offsetting	—	74 (14.31%)
No attribution	—	209 (40.43%)
Total Earnings Press Releases	696 (100.00%)	517 (100.00%)

Panel B: Attribution Patterns for Negative Surprises

Attribution Pattern	Number (Percentage) of Press Releases with Indicated Pattern	
	Seemingly Self-Serving Sample	Non-Self-Serving Sample
Defensive only	50 (15.29%)	—
Defensive and incentive-incompatible	45 (13.76%)	—
Defensive and offsetting	78 (23.85%)	—
Defensive, incentive-incompatible, and offsetting	154 (47.10%)	—
Incentive-incompatible only	—	69 (27.60%)
Offsetting only	—	49 (19.60%)
Incentive-incompatible and offsetting	—	39 (15.60%)
No attribution	—	93 (37.20%)
Total Earnings Press Releases	327 (100.00%)	250 (100.00%)

(continued on next page)

TABLE 2 (continued)

Panel C: Attribution Intensities for Positive Surprises

	Seemingly Self-Serving Sample		Non-Self-Serving Sample	
	Mean	[Median]	Mean	[Median]
Total number of attributions	6.28	[4.00]	1.41	[1.00]
% of total attributions that are seemingly self-serving	49.98%	[50.00%]	0.00%	[0.00%]
% of total attributions that are offsetting	31.03%	[33.33%]	68.29%	[100.00%]
% of total attributions that are incentive-incompatible	18.99%	[12.50%]	31.71%	[0.00%]

Panel D: Attribution Intensities for Negative Surprises

	Seemingly Self-Serving Sample		Non-Self-Serving Sample	
	Mean	[Median]	Mean	[Median]
Total number of attributions	7.06	[5.00]	1.46	[1.00]
% of total attributions that are seemingly self-serving	49.87%	[45.46%]	0.00%	[0.00%]
% of total attributions that are offsetting	31.11%	[33.33%]	43.52%	[33.33%]
% of total attributions that are incentive-incompatible	19.02%	[15.79%]	56.48%	[66.67%]

(continued on next page)

TABLE 2 (continued)

Panel E: Descriptive Statistics

Seemingly Self-Serving Sample							Non-Self-Serving Sample					
Variables	n	Mean	Std. Dev.	Lower Quartile	Median	Upper Quartile	n	Mean	Std. Dev.	Lower Quartile	Median	Upper Quartile
CAR	1,023	0.0010	0.0851	-0.0341	0.0000	0.0335	767	0.0096	0.1141	-0.0342	0.0015	0.0512
Surprise	1,023	0.0023	0.0463	-0.0010	0.0004	0.0024	767	-0.0075	0.0989	-0.0012	0.0001	0.0018
SurprisePos	1,023	0.0061	0.0418	0.0000	0.0004	0.0024	767	0.0028	0.0106	0.0000	0.0001	0.0018
SurpriseNeg	1,023	0.0038	0.0187	0.0000	0.0000	0.0010	767	0.0104	0.0980	0.0000	0.0000	0.0012
Size	1,023	6.5644	1.9369	5.3500	6.5307	7.7323	767	6.0407	1.7601	4.7551	5.8909	7.2987
Retvol	1,023	0.0317	0.0231	0.0174	0.0244	0.0391	767	0.0389	0.0254	0.0215	0.0323	0.0485
BTM	1,023	0.5883	0.6274	0.2681	0.4926	0.7106	767	0.5552	0.5129	0.2251	0.4659	0.7592
Common	984	0.2948	0.2501	0.0984	0.2273	0.4248	729	0.3015	0.2180	0.1195	0.2602	0.4349
BadQtr	959	0.3324	0.1422	0.2381	0.3243	0.4167	702	0.3188	0.1379	0.2222	0.3093	0.3913
Large	1,023	0.0156	0.1241	0.0000	0.0000	0.0000	767	0.0130	0.1135	0.0000	0.0000	0.0000
Loss	1,023	0.1554	0.3625	0.0000	0.0000	0.0000	767	0.2425	0.4289	0.0000	0.0000	0.0000
Qtr4	1,023	0.2317	0.4221	0.0000	0.0000	0.0000	767	0.2438	0.4297	0.0000	0.0000	0.0000
Positive	1,023	0.6804	0.4666	0.0000	1.0000	1.0000	767	0.6741	0.4690	0.0000	1.0000	1.0000

All variable definitions are in Panel B of Table 1.

(15.29 percent) of the press releases that report positive (negative) earnings surprises contain solely enhancing (defensive) attributions. Hence, seemingly self-serving attributions often appear alongside attributions that are either incentive-incompatible or that cite factors that offset the overall earnings news, perhaps because managers fear making the seemingly self-serving attributions easily refutable.¹³

Including press releases into the seemingly self-serving sample based on the existence of at least one seemingly self-serving attribution assumes that such press releases have an overall self-serving tenor. Panels C and D of Table 2 provide evidence on the relative intensity of different attribution types in our sample to corroborate this assumption. The mean (median) number of attributions for the seemingly self-serving sample is 6.28 (4.00) for positive surprises and 7.06 (5.00) for negative surprises. The non-self-serving sample has fewer attributions—the mean (median) for positive surprises is 1.41 (1.00) while the mean (median) for negative surprises is 1.46 (1.00). Seemingly self-serving attributions represent the most prevalent type of attribution for firms in the seemingly self-serving sample, averaging about 50 percent of the total attributions provided. Hence, seemingly self-serving attributions contribute in large part to the overall message of earnings press releases included in the seemingly self-serving sample.

IV. RESEARCH DESIGN

Test of the Differential Effects of Plausibility Measures on the Market’s Response to Earnings Announcements Containing Seemingly Self-Serving Attributions

H1 and H2 predict that investors use concurrent industry performance and a firm’s earnings commonality information when evaluating the plausibility of seemingly self-serving attributions, conditional on managers’ decisions to provide seemingly self-serving attributions.¹⁴ To test the hypotheses, we estimate the following regression on the seemingly self-serving sample:

$$\begin{aligned} CAR = & \beta_0 + \beta_1 Positive + \beta_2 SurprisePos + \beta_3 SurpriseNeg + \beta_4 SurprisePos \times BadQtr \\ & + \beta_5 SurprisePos \times Common + \beta_6 SurpriseNeg \times BadQtr + \beta_7 SurpriseNeg \\ & \times Common + \beta_8 BadQtr + \beta_9 Common + \gamma' Controls + \varepsilon. \end{aligned} \tag{1}$$

All variables are defined in Panel B of Table 1.

β_2 and β_3 in Equation (1) represent earnings response coefficients (ERC) for positive and negative earnings surprises, respectively. β_4 , β_5 , β_6 , and β_7 represent how market reactions to positive and negative earnings surprises vary with the two variables that we predict to be useful in assessing the plausibility of seemingly self-serving attributions.¹⁵

¹³ For earnings announcements in the non-self-serving sample with positive surprises, 209 (40.4 percent) contain no attributions, 67 (13.0 percent) contain only incentive-incompatible attributions, 167 (32.3 percent) contain only offsetting attributions, and 74 (14.3 percent) contain a combination of incentive-incompatible and offsetting attributions. For earnings announcements in the non-self-serving sample with negative surprises, 93 (37.2 percent) contain no attributions, 69 (27.6 percent) contain only incentive-incompatible attributions, 49 (19.6 percent) contain only offsetting attributions, and 39 (15.6 percent) contain a combination of incentive-incompatible and offsetting attributions.

¹⁴ Although our predictions do not extend to the subset of observations without self-serving attributions, we later use the non-self-serving sample as a comparison group in benchmarking the impacts of the plausibility measures for the seemingly self-serving sample.

¹⁵ Analyst forecasts are unavailable for only 122 observations out of a total of 1,790. Following Baginski et al. (2004) and Schrand and Walther (2000), we use a random walk model to measure earnings surprise when I/B/E/S consensus forecasts are unavailable. Inferences remain unchanged when we exclude these observations. Whenever we say inferences are unchanged, unaffected, or remain the same throughout the manuscript, we mean that the signs of the coefficients of interest are the same as those formally presented in the tables and that the coefficients of interest that are significant at the 5 percent level in the tables presented are also significant at the 5 percent level under alternative research design choices.

We use *BadQtr* to test H1 that investors view enhancing and defensive attributions as more believable when the concurrent industry performance is poor. We construct *BadQtr* as the percentage of previously announcing firms in the same six-digit Global Industry Classification Standard (GICS) code that announced earnings disappointments.¹⁶ We rely on the GICS codes to identify comparison firms facing similar economic circumstances because Bhojraj et al. (2003) find that GICS is superior to other industry classification schemes in identifying economically similar firms as measured by stock return comovements, valuation multiples, forecasted and realized growth rates, and various key financial ratios.¹⁷ We require a minimum of five prior announcing same-industry firms to calculate *BadQtr*. A finding of $\beta_4 > 0$ and $\beta_6 > 0$ would be consistent with H1.

We use *Common* to test H2 that investors view defensive (enhancing) attributions as more (less) believable as a firm's earnings commonality increases. Following Magee (1974), we measure a firm's earnings commonality by regressing the firm's return on assets (ROA) during the previous 20 quarters on average ROA of other firms in the industry for the same quarters to capture the firm's exposure to industry-wide influences, and average ROA for non-industry firms in Compustat to capture economy-wide influences.¹⁸ The R^2 from this regression reflects earnings commonality. A finding of $\beta_5 < 0$ and $\beta_7 > 0$ would support H2.

We control for standard economic determinants of earnings response coefficients (ERC). We control for firm size (*Size*) using the log of market value of equity as of the beginning of the quarter because Atiase (1985) and Freeman (1987) show that differences in predisclosure information as proxied by firm size also affect market reactions to earnings news. We control for firms' return volatility (*RetVol*) as a measure of risk and book-to-market ratio (*BTM*) as a measure of growth because Collins and Kothari (1989) and Easton and Zmijewski (1989) find that both growth prospects and risk affect market reactions to earnings news. Following Atiase, Supattarakul, and Tse (2005), we measure the book-to-market ratio as the book value of equity to the market value of equity at the beginning of the quarter, while return volatility is the standard deviation of daily returns estimated over the 100-trading-day window that ends two days before the quarter's earnings announcement. To control for the persistence of quarterly earnings surprises, we include *Loss*, an indicator variable that captures quarterly losses because Hayn (1995) shows that the ERC is smaller for loss firms than for profit firms. We control for large earnings surprises (*Large*) using an indicator variable that corresponds to absolute value of earnings surprises scaled by stock price greater than 0.1 because Freeman and Tse (1992) find that such extreme earnings surprises tend to be more temporary.

We also include an indicator variable for the fourth quarter of the year (*Qtr4*) to avoid confounding effects of the fourth quarter because firms often announce their annual earnings along with the fourth quarter earnings (Francis, Schipper, and Vincent 2002).¹⁹ In addition to controlling for the main effects of these control variables, we also include interaction terms between these

¹⁶ We only include industry peers that have announced earnings prior to our sample firms to capture the information that investors have available to them when interpreting our sample firms' earnings announcements.

¹⁷ Our inferences remain unchanged if we use SIC codes instead.

¹⁸ Earnings commonality is relevant in our context because investors can use the extent to which a firm's earnings has historically been driven by firm-specific factors, such as managerial actions, versus external common shocks to assess the plausibility of seemingly self-serving attributions of current earnings performance to either internal or external causes. For earnings commonality to serve this role, it is important to account for all drivers of earnings performance that are not firm-specific. Therefore, we include both industry and market indices in the calculation of *Common* based on prior evidence that both industry- and market-level forces drive a firm's earnings (Brown and Ball 1967; Magee 1974; Foster 1981).

¹⁹ Our inferences remain the same when we also control for the news contained in any management forecasts provided in the earnings press release and the interaction between signed discretionary accruals and positive/negative earnings surprises when earnings announcements also contain balance sheet information.

control variables and earnings surprises. Finally, we use the percentile-ranks of *BadQtr*, *Common*, *Size*, *RetVol*, and *BTM* scaled to range from 0 to 1 in our regressions to control for potential nonlinearities in the relations of these variables to ERCs and to aid interpretation of the interaction terms.

In addition, we estimate all regressions using ordinary least squares after eliminating outliers and clustering standard errors at both the firm and calendar quarter levels to correct for heteroscedasticity and dependence across observations (Froot 1989; Wooldridge 2002). We identify outliers as those observations with standardized residuals greater than 3 based on Huber M estimation, which is a robust estimation method that minimizes the sum of less rapidly increasing functions of the regression residuals instead of minimizing the sum of squared residuals (Huber 1973; Chen 2002).

Descriptive Statistics

Panel E of Table 2 presents descriptive statistics on firm characteristics. The seemingly self-serving and non-self-serving samples are similar in return volatility, book-to-market ratio, *BadQtr*, *Common*, and frequency of positive surprises and large surprises. Compared to the seemingly self-serving sample, the non-self-serving sample has smaller market values of equity, larger market reactions to earnings news, and higher frequency of losses.

V. RESULTS

Results of Testing the Differential Effects of Plausibility Measures on the Market's Response to Earnings Announcements Containing Seemingly Self-Serving Attributions

Table 3 presents the results of our hypothesis testing. The positive coefficient on *SurprisePos* $\times$ *BadQtr* indicates greater market rewards to positive earnings surprises accompanied by enhancing attributions when more of the previously announcing industry peers released bad earnings news ($\beta_4 = 2.96$, $p < 0.01$), supporting H1a. Consistent with H1b, the positive coefficient on *SurpriseNeg* $\times$ *BadQtr* indicates less severe market penalties to negative earnings surprises accompanied by defensive attributions when more of the previously announcing industry peers released bad earnings news ($\beta_6 = 2.57$, $p < 0.01$).

The results also support our predictions for earnings commonality (H2a and H2b). Specifically, the negative coefficient on *SurprisePos* $\times$ *Common* ($\beta_5 = -3.73$; $p < 0.01$) indicates that market rewards to positive surprises accompanied by enhancing attributions decline as earnings commonality increases, suggesting that a higher degree of exposure to common industry and market forces hinders a firm's ability to take credit for favorable earnings performance. The positive coefficient on *SurpriseNeg* $\times$ *Common* ($\beta_7 = 1.25$; $p = 0.04$) indicates that market penalties to bad earnings news accompanied by defensive attributions decline as earnings commonality increases, suggesting that a higher degree of exposure to common industry and market forces makes it more believable that current earnings disappointments are due to external causes.

To gauge the economic significance of our findings, we use the coefficient estimates from Table 3 to calculate the possible range of estimated abnormal announcement returns for a firm with the average positive or negative surprise when it provides a seemingly self-serving attribution under the lowest plausibility versus the highest plausibility conditions, holding control variables constant at their medians. In untabulated analysis, we find that the predicted abnormal return for a firm with an average positive surprise when it provides an enhancing attribution is -0.79 percent under the lowest plausibility conditions (i.e., when *BadQtr* = 0 and *Common* = 1) and 4.77 percent under the highest (i.e., when *BadQtr* = 1 and *Common* = 0). We further find that the predicted abnormal return for a firm with an average negative surprise when it provides a defensive attribution is -5.11

TABLE 3

Regression Analysis of the Impacts of Plausibility Measures on the Market’s Response to Earnings Press Releases Accompanied by Seemingly Self-Serving Attributions

$$CAR = \beta_0 + \beta_1 Positive + \beta_2 SurprisePos + \beta_3 SurpriseNeg + \beta_4 SurprisePos \times BadQtr + \beta_5 SurprisePos \times Common + \beta_6 SurpriseNeg \times BadQtr + \beta_7 SurpriseNeg \times Common + \beta_8 BadQtr + \beta_9 Common + \gamma' Controls + \varepsilon. \text{ (Model 1)}$$

Variable	Predicted Sign	Coefficient Estimate	t-statistic	p-value
Intercept		−0.0134	−1.52	0.14
Positive		0.0238	4.31	< 0.01
SurprisePos	+	1.1738	2.87	< 0.01
SurpriseNeg	−	−1.2563	−1.79	0.08
Impacts of Plausibility Factors				
SurprisePos × BadQtr	+	2.9556	7.93	< 0.01
SurprisePos × Common	−	−3.7349	−10.08	< 0.01
SurpriseNeg × BadQtr	+	2.5734	4.58	< 0.01
SurpriseNeg × Common	+	1.2467	2.18	0.04
BadQtr		−0.0083	−3.40	< 0.01
Common		−0.0012	−0.18	0.86
Controls				
SurprisePos × Size		1.1383	5.66	< 0.01
SurprisePos × Retvol		0.3853	0.90	0.38
SurprisePos × BTM		−0.2390	−1.13	0.27
SurprisePos × Large		−0.2611	−1.30	0.20
SurprisePos × Loss		−1.1089	−6.64	< 0.01
SurprisePos × Qtr4		1.2544	3.45	< 0.01
SurpriseNeg × Size		−2.2056	−1.78	0.08
SurpriseNeg × Retvol		−1.6473	−1.91	0.07
SurpriseNeg × BTM		0.0123	0.03	0.98
SurpriseNeg × Large		0.0373	0.12	0.91
SurpriseNeg × Loss		0.9453	2.22	0.03
SurpriseNeg × Qtr4		0.2509	0.55	0.59
Size		0.0027	0.41	0.68
Retvol		−0.0099	−1.28	0.21
BTM		0.0080	1.27	0.21
Large		0.0185	0.58	0.57
Loss		−0.0119	−1.69	0.10
Qtr4		−0.0050	−1.15	0.26
R ²		21.29%		
n		935		

Table 3 presents the results of estimating Equation (1) on the seemingly self-serving sample using OLS regression after excluding observations with standardized residuals greater than 3 using the Huber M procedure. Standard errors are clustered by firm and by calendar quarter. p-values are two-tailed. All variable definitions are in Panel B of Table 1.

percent under the lowest plausibility conditions (i.e., when *BadQtr* = 0 and *Common* = 0) and −1.42 percent under the highest (i.e., when *BadQtr* = 1 and *Common* = 1).

The evidence in Table 3 suggests that investors rely on the plausibility measures to differentiate the seemingly self-serving attributions when responding to earnings news. However, it is possible that investors' *perceptions* are biased. We therefore analyze whether investors' perceptions based on these measures are consistent with their indications for the *actual* persistence of earnings surprises. Specifically, we estimate the following model:

$$\begin{aligned} \text{FutureROA} = & \beta_0 + \beta_1 \text{Positive} + \beta_2 \text{SurprisePos} + \beta_3 \text{SurpriseNeg} + \beta_4 \text{SurprisePos} \\ & \times \text{BadQtr} + \beta_5 \text{SurprisePos} \times \text{Common} + \beta_6 \text{SurpriseNeg} \times \text{BadQtr} \\ & + \beta_7 \text{SurpriseNeg} \times \text{Common} + \beta_8 \text{BadQtr} + \beta_9 \text{Common} + \beta_{10} \text{LagROA} + \varepsilon. \end{aligned} \quad (2)$$

All variables are defined in Panel B of Table 1.

β_2 and β_3 in Equation (2) represent the realized persistence of positive and negative earnings surprises, respectively. β_4 to β_7 capture the interaction terms that allow the persistence of both positive and negative earnings surprises to vary by the plausibility measures. Earlier results in Table 3 indicate that investors respond as though positive (negative) surprises accompanied by seemingly self-serving attributions are (1) more (less) persistent during industry-wide bad quarters, and (2) less persistent as earnings commonality increases. If investors' responses are consistent with the associations between these plausibility measures and the actual persistence of the earnings surprises, then we expect positive earnings surprises accompanied by more plausible enhancing attributions to be more persistent and negative surprises accompanied by more plausible defensive attributions to be less persistent (i.e., $\beta_4 > 0$, $\beta_5 < 0$, $\beta_6 > 0$, and $\beta_7 > 0$).²⁰

Table 4 reports the results of estimating Equation (2). We find a positive coefficient on *SurprisePos* × *BadQtr* ($\beta_4 = 0.17$; $p < 0.01$) and a positive coefficient on *SurpriseNeg* × *BadQtr* ($\beta_6 = 0.13$; $p < 0.01$), indicating that the market's use of *BadQtr* in response to earnings news is directionally consistent with this variable's association with the actual persistence of earnings surprises. Similarly, the negative coefficient on *SurprisePos* × *Common* ($\beta_5 = -0.63$; $p < 0.01$) and the positive coefficient on *SurpriseNeg* × *Common* ($\beta_7 = 0.16$; $p < 0.01$) indicate that the market's use of *Common* in response to earnings news is also directionally consistent with its association with the actual persistence of earnings surprises.

Supplemental Analysis on the Impacts of Plausibility Factors in the Non-Self-Serving Sample

The evidence to this point demonstrates the impacts of the plausibility factors on the market's reactions to earnings news when earnings press releases contain seemingly self-serving attributions. To bolster our interpretation of *Common* and *BadQtr* as plausibility factors, we compare the impacts of *Common* and *BadQtr* for the seemingly self-serving sample, where plausibility concerns are heightened, to their impacts for the non-self-serving sample. We first develop a model of the decision to provide seemingly self-serving attributions (*Selfserving* = 1) to control for possible endogeneity in our comparative analyses. The endogeneity issue arises because managers can *choose* whether to provide a seemingly self-serving attribution. Therefore, it is possible that managers base their decision at least partially on factors that also affect market reactions to earnings

²⁰ Recall that we measure *SurpriseNeg* as the absolute value of earnings surprise when earnings surprise is less than 0 to ease interpretation in the market reaction tests. So a positive coefficient on *SurpriseNeg* × *BadQtr* (β_6) or *SurpriseNeg* × *Common* (β_7) in the estimation of Equation (2) indicates that negative surprises are *less* persistent.

TABLE 4

Regression Analysis of the Information Content of the Plausibility Measures Regarding the Actual Persistence of Earnings Surprises for Earnings Announcements Accompanied by Seemingly Self-Serving Attributions

$$\begin{aligned} \text{FutureROA} = & \beta_0 + \beta_1 \text{Positive} + \beta_2 \text{SurprisePos} + \beta_3 \text{SurpriseNeg} + \beta_4 \text{SurprisePos} \times \text{BadQtr} \\ & + \beta_5 \text{SurprisePos} \times \text{Common} + \beta_6 \text{SurpriseNeg} \times \text{BadQtr} \\ & + \beta_7 \text{SurpriseNeg} \times \text{Common} + \beta_8 \text{BadQtr} + \beta_9 \text{Common} + \beta_{10} \text{LagROA} \\ & + \varepsilon. \text{ (Model 2)} \end{aligned}$$

Variable	Coefficient Estimate	t-statistic	p-value
Intercept	0.0000	0.05	0.96
Positive	0.0007	2.24	0.03
SurprisePos	0.2000	28.32	< 0.01
SurpriseNeg	−0.2290	−7.87	< 0.01
Impacts of Plausibility Factors			
SurprisePos × BadQtr	0.1685	3.76	< 0.01
SurprisePos × Common	−0.6309	−16.97	< 0.01
SurpriseNeg × BadQtr	0.1321	6.52	< 0.01
SurpriseNeg × Common	0.1617	4.82	< 0.01
BadQtr	−0.0011	−2.79	< 0.01
Common	0.0010	1.32	0.20
LagROA	0.9789	78.06	< 0.01
R ²		95.07%	
n		925	

Table 4 presents the results of estimating Equation (2) on the seemingly self-serving sample using OLS regression after excluding observations with standardized residuals greater than 3 using the Huber M procedure. Standard errors are clustered by firm and by calendar quarter. p-values are two-tailed. All variable definitions are in Panel B of Table 1.

news, which creates the potential for correlation between the *Selfserving* indicator variable and the error term in market reaction tests that combine the seemingly self-serving and non-self-serving samples. We apply the Heckman (1979) procedure to deal with this potential endogeneity issue. As a first step, we estimate the following probit model:

$$\begin{aligned} \text{Prob}(\text{Selfserving}) = & \beta_0 + \beta_1 \text{Positive} + \beta_2 \text{Large} + \beta_3 \text{Size} + \beta_4 \text{Numsegments} + \beta_5 \text{Hightech} \\ & + \beta_6 \text{Regulated} + \beta_7 \text{IS.trade} + \beta_8 \text{ForecastBias} + \beta_9 \text{AF} + \beta_{10} \text{IO\%} \\ & + \beta_{11} \text{NegEarningsChg} + \beta_{12} \text{M \& A} + \beta_{13} \text{Restructure} + \beta_{14} \text{\%}\Delta \text{GDP} \\ & + \beta_{15} \Delta \text{Exchangerates} + \beta_{16} \Delta \text{Interestrates} + \beta_{17} \Delta \text{Inflation} \\ & + \beta_{18} \text{Plaus.Badqtr} + \beta_{19} \text{Plaus.Common} + \beta_{20} \text{TimeTrend} + \varepsilon. \end{aligned}$$

(3)

All variables are defined in Panel B of Table 1.

Given the lack of prior evidence and theory on why firms choose to provide seemingly self-serving attributions, the variables we include in the probit model are exploratory in nature. We include firm-specific determinants to capture the possibility that individual firms systematically use seemingly self-serving attributions in earnings press releases and time-varying determinants to

capture the possibility that firms dynamically alter their use of seemingly self-serving attributions in response to changing circumstances.²¹

Specifically, we include earnings attributes such as the sign of the news (*Positive*) and an indicator variable for large earnings surprises (*Large*) because they are key determinants of the market reaction that managers intend to influence. We consider firm-specific factors that are established determinants of disclosure decisions such as industry membership (*Hightech* and *Regulated*), which captures the amount of litigation risk and regulatory scrutiny a firm faces (Skinner 1994; Baginski et al. 2004), and *Size*, which captures the richness of a firm's information environment. The number of business segments (*Numsegments*) controls for the complexity of a firm's operations, which may need further explanation from management. Insider trading surrounding the earnings announcement (*IS_trade*) captures managers' strategic motivations. Analyst following (*AF*) and institutional ownership (*IO%*) proxy for capital market scrutiny. We include the existence of optimistic management forecasts (*ForecastBias*) to capture managers' psychological biases, such as managerial optimism, which may affect their tendency to provide seemingly self-serving attributions. We include current period events that are likely explanations for current performance including seasonal earnings changes (*NegEarningsChg*), recent mergers and acquisitions (*M&A*), and recent restructurings and/or discontinued operations (*Restructure*) as reflected in special items (Francis and Ke 2006).

We also include macroeconomic news that may help explain corporate performance such as current quarter changes in interest rates (Δ *Interestrates*), exchange rates (Δ *Exchangerates*), inflation rates (Δ *Inflation*), and seasonal changes in GDP (Δ *GDP*). We include *ex ante* plausibility measures (*Plaus_BadQtr* and *Plaus_Common*) because managers may consider how likely investors will believe a seemingly self-serving attribution before providing it. Finally, we include a quarterly time trend (*TimeTrend*) to account for any systematic changes over time in the percentage of earnings announcements that contain seemingly self-serving attributions.²²

Panel A of Table 5 provides descriptive statistics for the variables used in Equation (3). We note a number of significant univariate differences between the two samples. In terms of firm-specific determinants, observations in the seemingly self-serving sample tend to be more heavily weighted toward larger firms, firms with more segments, more analyst following, and higher institutional ownership, but are less likely to be in high-tech and regulated industries. For time-varying determinants, seemingly self-serving attributions occur less often in periods with negative seasonal earnings changes and in periods with recent M&A or restructuring activities.

Panel B of Table 5 reports the results of estimating Equation (3). We find that firms are less likely to provide seemingly self-serving attributions when seasonal earnings changes are negative (*NegEarningsChg*) ($p = 0.03$), when they are in high-tech or regulated industries ($p \leq 0.03$), or

²¹ To provide a sense of the relative importance of firm-specific and time-varying factors in explaining the incidence of seemingly self-serving attributions, we group our sample firms into three categories: (1) those that provide seemingly self-serving attributions frequently, (2) those that provide seemingly self-serving attributions rarely, and (3) those that are less consistent in providing seemingly self-serving attributions. We define firms that provide self-serving attributions frequently (rarely) as those that provide seemingly self-serving attributions in 2/3 or more (1/3 or less) of their earnings press releases contained in the sample. We characterize all other firms as being less consistent in their use of seemingly self-serving attributions. Based on these criteria, 28 firms (or 29 percent) provide seemingly self-serving attributions frequently, 22 firms (or 22 percent) provide them rarely, and 48 firms (or 49 percent) provide them less consistently. Hence, 51 percent of the firms follow a consistent practice of either frequently or rarely providing seemingly self-serving attributions in earnings press releases while 49 percent of the firms are less consistent in providing seemingly self-serving attributions. It appears that the observed use of seemingly self-serving attributions has both a firm-specific component and a time-varying component. So we include both sets of determinants into Equation (3).

²² Specifically, in an untabulated regression of the percentage of quarterly earnings announcements with a self-serving attribution on a time trend, the coefficient on the quarterly time trend is significantly positive ($p < 0.01$).

TABLE 5
Analysis of Firms' Choice to Provide a Seemingly Self-Serving Attribution in Earnings Press Releases

Panel A: Descriptive Statistics

Variables	Seemingly Self-Serving Sample			Non-Self-Serving Sample			Test of Differences		Combined Sample		
	n	Mean	Median	n	Mean	Median	Mean	Median	n	Mean	Median
Size	1,023	6.564	6.531	767	6.041	5.891	***	†††	1,790	6.340	6.300
Numsegments	1,023	2.040	1.000	767	1.703	1.000	***	†††	1,790	1.896	1.000
IS_trade	1,023	0.316	0.000	767	0.278	0.000	*	†	1,790	0.299	0.000
ForecastBias	1,023	0.053	0.000	767	0.074	0.000	*	†	1,790	0.062	0.000
AF	1,023	5.198	3.000	767	4.511	3.000	***	†††	1,790	4.903	3.000
IO%	1,023	0.505	0.539	767	0.458	0.440	***	†††	1,790	0.485	0.493
%ΔGDP	1,023	4.275	4.132	767	4.209	4.132	*	n.s.	1,790	4.247	4.132
ΔExchangerates	1,023	−0.059	0.490	767	0.115	0.590	n.s.	n.s.	1,790	0.015	0.578
ΔInterestrates	1,023	−0.024	0.040	767	−0.065	0.070	n.s.	n.s.	1,790	−0.042	0.060
ΔInflation	1,023	1.233	1.200	767	1.194	1.200	n.s.	n.s.	1,790	1.216	1.200
NegEarningsChg	1,023	0.356	0.000	767	0.436	0.000	***	†††	1,790	0.390	0.000
Positive	1,023	0.680	1.000	767	0.674	1.000	n.s.	n.s.	1,790	0.678	1.000
Large	1,023	0.016	0.000	767	0.013	0.000	n.s.	n.s.	1,790	0.015	0.000
Hightech	1,023	0.167	0.000	767	0.299	0.000	***	†††	1,790	0.224	0.000
Regulated	1,023	0.034	0.000	767	0.054	0.000	**	††	1,790	0.043	0.000
M&A	1,023	0.130	0.000	767	0.189	0.000	***	†††	1,790	0.155	0.000
Restructure	1,023	0.094	0.000	767	0.141	0.000	***	†††	1,790	0.114	0.000
Plaus_BadQtr	959	0.514	1.000	702	0.472	0.000	*	†	1,661	0.496	0.000
Plaus_Common	984	0.497	0.000	729	0.486	0.000	n.s.	n.s.	1,713	0.492	0.000

Panel B: Probit Regression of the Likelihood of Seemingly Self-Serving Attribution in Press Releases

$$\begin{aligned} Prob(Selfserving) = & \beta_0 + \beta_1 Positive + \beta_2 Large + \beta_3 Size + \beta_4 Numsegments + \beta_5 Hightech \\ & + \beta_6 Regulated + \beta_7 IS\_trade + \beta_8 ForecastBias + \beta_9 AF + \beta_{10} IO\% \\ & + \beta_{11} NegEarningsChg + \beta_{12} M \& A + \beta_{13} Restructure + \beta_{14} \% \Delta GDP \\ & + \beta_{15} \Delta Exchangerates + \beta_{16} \Delta Interestrates + \beta_{17} \Delta Inflation \\ & + \beta_{18} Plaus\_Badqtr + \beta_{19} Plaus\_Common + \beta_{20} TimeTrend \\ & + \varepsilon. \text{ (Model 3)} \end{aligned}$$

Variables	Coefficient Estimate	z-statistic	p-value
Intercept	−0.5286	−1.86	0.06
Earnings News Characteristics			
Positive	−0.0277	−0.38	0.70
Large	0.3500	1.01	0.31
Firm Characteristics			
Size	0.0759	2.66	< 0.01
Numsegments	0.0778	2.79	< 0.01
Hightech	−0.4567	−5.48	< 0.01
Regulated	−0.3520	−2.24	0.03

(continued on next page)

TABLE 5 (continued)

Variables	Coefficient Estimate	z-statistic	p-value
<i>IS_trade</i>	−0.0237	−0.32	0.75
<i>ForecastBias</i>	−0.1887	−1.28	0.20
<i>AF</i>	0.0050	0.56	0.57
<i>IO%</i>	−0.1124	−0.74	0.46
Period-Specific Events			
<i>NegEarningsChg</i>	−0.1549	−2.20	0.03
<i>M&A</i>	−0.2209	−2.28	0.02
<i>Restructure</i>	−0.1560	−1.42	0.16
Macroeconomic News			
<i>%ΔGDP</i>	0.0467	0.91	0.36
<i>ΔExchangerates</i>	0.0114	0.75	0.46
<i>ΔInterestrates</i>	0.0150	0.20	0.84
<i>ΔInflation</i>	0.0196	0.37	0.71
Ex ante Plausibility			
<i>Plaus_BadQtr</i>	0.0459	0.69	0.49
<i>Plaus_Common</i>	−0.0048	−0.07	0.94
Trend			
<i>TimeTrend</i>	0.0144	2.90	< 0.01
Pseudo R ²		6.26%	
n		1,606	

*(†), ** (††),***(†††) Indicate significance of differences in mean (median) at 10 percent, 5 percent, and 1 percent levels, respectively, based on t-tests (nonparametric Wilcoxon rank-sum tests).
Table 5 reports results that are based on a probit estimation on the combined seemingly self-serving and non-self-serving samples. Robust standard errors are reported. n.s. indicates that the difference is not significant at conventional levels. All variable definitions are in Panel B of Table 1.

when they recently completed an M&A ($p = 0.02$). On the other hand, firms are more likely to provide seemingly self-serving attributions when they are larger and have more business segments ($p < 0.01$). In addition, firms have become more likely to provide seemingly self-serving attributions over time (*TimeTrend*) ($p < 0.01$). Insider trading or managerial optimism are not associated with the likelihood of seemingly self-serving attributions, indicating that opportunism or psychological biases might not be the main reasons for managers to provide seemingly self-serving attributions.^{23,24}

Following Heckman (1979), we use the inverse Mills ratio (*Lambda*) obtained from estimating Equation (3) to correct for endogeneity in the models below that compare the impacts of the plausibility factors for the seemingly self-serving versus the non-self-serving samples:

²³ However, an alternative explanation could be that our measures do not adequately capture managerial opportunism or psychological bias. Given the scope of our study, we leave it for future research to develop finer measures for these constructs.

²⁴ In untabulated sensitivity analysis, we allow the coefficients on the macroeconomic variables as well as the M&A and restructuring variables to vary by the sign of earnings news by interacting these variables with *Positive*. The coefficients on the interactions that involve the macroeconomic variables are insignificant at the 5 percent level. Both M&A and restructuring are associated with a lower likelihood of seemingly self-serving attributions when earnings news is negative, because the coefficients on the main effects of both variables are significantly negative. The effects of M&A and restructuring on the likelihood of seemingly self-serving attributions are significantly different when earnings news is positive, because the coefficients on the interactions that involve these variables are significantly positive. Using the inverse Mills ratio calculated after incorporating the additional interactions does not affect our inferences from our remaining tests.

$$\begin{aligned}
CAR = & \beta_0 + \beta_1 Positive + \beta_2 SurprisePos + \beta_3 SurpriseNeg + \beta_4 SurprisePos \times BadQtr \\
& + \beta_5 SurprisePos \times Common + \beta_6 SurpriseNeg \times BadQtr + \beta_7 SurpriseNeg \\
& \times Common + \beta_8 BadQtr + \beta_9 Common + \beta_{10} SurprisePos \times Selfserving \\
& + \beta_{11} SurpriseNeg \times Selfserving + \beta_{12} SurprisePos \times BadQtr \times Selfserving \\
& + \beta_{13} SurprisePos \times Common \times Selfserving + \beta_{14} SurpriseNeg \times BadQtr \\
& \times Selfserving + \beta_{15} SurpriseNeg \times Common \times Selfserving + \beta_{16} Selfserving \\
& + \beta_{17} Lambda + \gamma' Controls + \varepsilon.
\end{aligned} \tag{4}$$

$$\begin{aligned}
FutureROA = & \beta_0 + \beta_1 Positive + \beta_2 SurprisePos + \beta_3 SurpriseNeg + \beta_4 SurprisePos \\
& \times BadQtr + \beta_5 SurprisePos \times Common + \beta_6 SurpriseNeg \times BadQtr \\
& + \beta_7 SurpriseNeg \times Common + \beta_8 BadQtr + \beta_9 Common + \beta_{10} LagROA \\
& + \beta_{11} SurprisePos \times Selfserving + \beta_{12} SurpriseNeg \times Selfserving \\
& + \beta_{13} SurprisePos \times BadQtr \times Selfserving + \beta_{14} SurprisePos \times Common \\
& \times Selfserving + \beta_{15} SurpriseNeg \times BadQtr \times Selfserving + \beta_{16} SurpriseNeg \\
& \times Common \times Selfserving + \beta_{17} Selfserving + \beta_{18} Lambda + \varepsilon.
\end{aligned} \tag{5}$$

All variables are defined in Panel B of Table 1.

Equations (4) and (5) augment Equations (1) and (2), respectively, with three-way interaction terms that allow the impacts of the plausibility factors for the seemingly self-serving and non-self-serving samples to differ. If the effects of the plausibility factors documented in Table 3 reflect the unique roles these factors play in evaluating seemingly self-serving attributions, then we expect $\beta_{12} > 0$, $\beta_{13} < 0$, $\beta_{14} > 0$, $\beta_{15} > 0$ in Equation (4), and $\beta_{13} > 0$, $\beta_{14} < 0$, $\beta_{15} > 0$, $\beta_{16} > 0$ in Equation (5).

We follow Lennox, Francis, and Wang's (2012) suggestion to evaluate our systems of equations for exclusion restrictions wherein the second-stage models in Equations (4) and (5) should exclude variables that are significant in the first-stage model in Equation (3) but are unrelated to the dependent variables in the second-stage models. We note that Equation (3) contains a number of variables, such as *Numsegments*, *Hightech*, *Regulated*, *M&A*, and *TimeTrend*, that are significant determinants of the decision to provide seemingly self-serving attributions but have no theorized effect on either announcement returns or future ROA. We therefore use these variables to meet the exclusion restrictions and as instrumental variables for identifying the systems of equations. We also note that multicollinearity is not an issue because the variance inflation factor on *Lambda* is less than 2.5.

Table 6 presents the results of estimating Equation (4). We find that the impacts of the plausibility factors for the non-self-serving sample are either insignificant (*SurprisePos* $\times$ *Common* and *SurpriseNeg* $\times$ *Common*) or opposite in sign to the corresponding effects for the seemingly self-serving sample (*SurprisePos* $\times$ *BadQtr* and *SurpriseNeg* $\times$ *BadQtr*). More importantly, the impacts of the plausibility factors for the seemingly self-serving sample are always significantly different from their impacts for the non-self-serving sample, as reflected in the coefficients on the three-way interaction terms, which are all significant at $p \leq 0.02$. These findings indicate that investors use the plausibility factors in the predicted directions only when managers provide seemingly self-serving attributions.

Table 7 reports the estimation results for Equation (5). We find that the impacts of the plausibility factors for the non-self-serving sample are either insignificant (*SurprisePos* $\times$ *BadQtr* and *SurprisePos* $\times$ *Common*), marginally significant (*SurpriseNeg* $\times$ *BadQtr*), or opposite in sign to the corresponding effects for the seemingly self-serving sample (*SurpriseNeg* $\times$ *Common*).

TABLE 6

Regression Analysis of the Impacts of the Plausibility Factors for Earnings Announcements With and Without Seemingly Self-Serving Attributions

$$\begin{aligned} CAR = & \beta_0 + \beta_1 Positive + \beta_2 SurprisePos + \beta_3 SurpriseNeg + \beta_4 SurprisePos \times BadQtr \\ & + \beta_5 SurprisePos \times Common + \beta_6 SurpriseNeg \times BadQtr + \beta_7 SurpriseNeg \\ & \times Common + \beta_8 BadQtr + \beta_9 Common + \beta_{10} SurprisePos \times Selfserving \\ & + \beta_{11} SurpriseNeg \times Selfserving + \beta_{12} SurprisePos \times BadQtr \times Selfserving \\ & + \beta_{13} SurprisePos \times Common \times Selfserving + \beta_{14} SurpriseNeg \times BadQtr \\ & \times Selfserving + \beta_{15} SurpriseNeg \times Common \times Selfserving + \beta_{16} Selfserving \\ & + \beta_{17} Lambda + \gamma Controls + \varepsilon. \text{ (Model 4)} \end{aligned}$$

Variable	Predicted Sign	Coefficient Estimate	t-statistic	p-value
Intercept		0.0134	1.10	0.28
Positive		0.0172	4.45	< 0.01
SurprisePos	+	2.1586	2.48	0.02
SurpriseNeg	−	−0.4006	−0.98	0.33
Impacts of Plausibility Factors for Non-Self-Serving Sample				
SurprisePos × BadQtr		−1.6752	−2.35	0.03
SurprisePos × Common		−1.1376	−1.63	0.11
SurpriseNeg × BadQtr		−1.0475	−4.63	< 0.01
SurpriseNeg × Common		0.3910	1.11	0.27
BadQtr		−0.0046	−1.19	0.24
Common		0.0005	0.11	0.91
Incremental Impacts for Seemingly Self-Serving Sample				
SurprisePos × Selfserving		−0.1661	−0.29	0.77
SurpriseNeg × Selfserving		−2.5651	−8.95	< 0.01
SurprisePos × BadQtr × Selfserving	+	4.7032	7.60	< 0.01
SurprisePos × Common × Selfserving	−	−3.1761	−3.66	< 0.01
SurpriseNeg × BadQtr × Selfserving	+	2.9542	8.66	< 0.01
SurpriseNeg × Common × Selfserving	+	1.4179	2.51	0.02
Selfserving		−0.0047	−1.50	0.17
Lambda		−0.0188	−1.79	0.08
Controls				
SurprisePos × Size		1.3815	7.91	< 0.01
SurprisePos × Retvol		−0.3164	−0.50	0.62
SurprisePos × BTM		−0.1023	−0.49	0.63
SurprisePos × Large		−0.5100	−1.75	0.09
SurprisePos × Loss		−1.2517	−5.23	< 0.01
SurprisePos × Qtr4		0.7808	2.02	0.05
SurpriseNeg × Size		−0.3507	−1.95	0.06
SurpriseNeg × Retvol		0.0987	0.22	0.83
SurpriseNeg × BTM		0.6004	1.38	0.18
SurpriseNeg × Large		−0.9375	−2.62	< 0.01
SurpriseNeg × Loss		0.6533	2.41	0.02
SurpriseNeg × Qtr4		0.3902	2.66	0.01
Size		−0.0109	−1.23	0.23

(continued on next page)

TABLE 6 (continued)

Variable	Predicted Sign	Coefficient Estimate	t-statistic	p-value
<i>Retvol</i>		−0.0057	−0.58	0.56
<i>BTM</i>		0.0024	0.37	0.72
<i>Large</i>		0.1593	3.77	< 0.01
<i>Loss</i>		−0.0084	−1.77	0.09
<i>Qtr4</i>		−0.0028	−0.60	0.55
R ²		23.54%		
n		1,606		

Table 6 presents the results of estimating Equation (4) on the combined sample using OLS regression after excluding observations with standardized residuals greater than 3 using the Huber M procedure. Standard errors are clustered by firm and by calendar quarter. p-values are two-tailed. All variable definitions are in Panel B of Table 1.

Although the impact of *BadQtr* does not differ between the two samples, we find that the impact of *Common* is significantly different for the seemingly self-serving sample, as reflected in the coefficients on *SurprisePos* × *Common* × *Selfserving* and *SurpriseNeg* × *Common* × *Selfserving* ($p < 0.01$). This result indicates that *Common* has incremental information about the persistence of earnings surprises accompanied by seemingly self-serving attributions.

Taken together, the evidence in Tables 6 and 7 indicates that the pattern of the effects of *Common* and *BadQtr* for the seemingly self-serving sample documented in Tables 3 and 4 does not extend to the non-self-serving sample. This evidence bolsters our interpretation of these variables as unique indicators of the plausibility of seemingly self-serving attributions.

Characteristics of Firms that Provide Less Plausible Seemingly Self-Serving Attributions

Our evidence that investors rely on relevant information to distinguish the plausibility of seemingly self-serving attributions leads to a natural question: Why would managers provide seemingly self-serving attributions when investors may view them as implausible?²⁵ While we leave a full exploration of this issue to future work, we provide preliminary insight by comparing the characteristics of firms that provide less plausible seemingly self-serving attributions with those in the non-self-serving sample. We identify a firm that provides less plausible seemingly self-serving attributions as one that reports: (1) a negative surprise and its *BadQtr* and *Common* are both below the sample median, or (2) a positive surprise and its *BadQtr* is below the sample median and *Common* is at or above the sample median. In untabulated analysis, we find that firms that provide less plausible attributions are larger and have higher likelihood of insider trading around earnings announcements, higher analyst following, higher institutional ownership, higher return volatility, and lower book-to-market ratio ($p < 0.05$). These findings imply that managers with insider trading incentives and those facing greater capital market scrutiny are more likely to offer seemingly self-

²⁵ Managers may do so if they believe investors are naive (Bamber, Jiang, Petroni, and Wang 2010) or if they are unaware of the information that investors consult when assessing plausibility. Since managers cannot perfectly foresee investors' responses to seemingly self-serving attributions, what matters is what managers believe at the time they offer seemingly self-serving attributions. If managers believe there is a chance that investors might be persuaded by their (implausible) seemingly self-serving attributions, then they are more likely to offer them even if *ex post* it turns out that investors can see through such implausible attributions.

TABLE 7

Regression Analysis of the Incremental Information Content of the Plausibility Measures regarding the Persistence of Earnings Surprises for the Seemingly Self-Serving Sample

$$\begin{aligned} \text{FutureROA} = & \beta_0 + \beta_1 \text{Positive} + \beta_2 \text{SurprisePos} + \beta_3 \text{SurpriseNeg} + \beta_4 \text{SurprisePos} \\ & \times \text{BadQtr} + \beta_5 \text{SurprisePos} \times \text{Common} + \beta_6 \text{SurpriseNeg} \times \text{BadQtr} \\ & + \beta_7 \text{SurpriseNeg} \times \text{Common} + \beta_8 \text{BadQtr} + \beta_9 \text{Common} + \beta_{10} \text{LagROA} \\ & + \beta_{11} \text{SurprisePos} \times \text{Selfserving} + \beta_{12} \text{SurpriseNeg} \times \text{Selfserving} \\ & + \beta_{13} \text{SurprisePos} \times \text{BadQtr} \times \text{Selfserving} + \beta_{14} \text{SurprisePos} \times \text{Common} \\ & \times \text{Selfserving} + \beta_{15} \text{SurpriseNeg} \times \text{BadQtr} \times \text{Selfserving} + \beta_{16} \text{SurpriseNeg} \\ & \times \text{Common} \times \text{Selfserving} + \beta_{17} \text{Selfserving} + \beta_{18} \text{Lambda} + \varepsilon. \text{ (Model 5)} \end{aligned}$$

Variable	Coefficient Estimate	t-statistic	p-value
Intercept	0.0026	2.93	< 0.01
Positive	0.0012	2.19	0.04
SurprisePos	0.1255	0.79	0.44
SurpriseNeg	0.1286	5.48	< 0.01
Impacts of Plausibility Factors for Non-Self-Serving Sample			
SurprisePos × BadQtr	−0.0242	−0.10	0.92
SurprisePos × Common	−0.0591	−0.36	0.72
SurpriseNeg × BadQtr	0.2104	1.73	0.09
SurpriseNeg × Common	−0.4845	−2.78	< 0.01
BadQtr	−0.0019	−4.41	< 0.01
Common	0.0008	1.03	0.31
LagROA	0.9604	86.66	< 0.01
Incremental Impacts for Seemingly Self-Serving Sample			
SurprisePos × Selfserving	0.1135	0.72	0.48
SurpriseNeg × Selfserving	−0.2960	−6.86	< 0.01
SurprisePos × BadQtr × Selfserving	0.1987	0.75	0.46
SurprisePos × Common × Selfserving	−0.6205	−3.66	< 0.01
SurpriseNeg × BadQtr × Selfserving	0.0144	0.12	0.91
SurpriseNeg × Common × Selfserving	0.5408	3.16	< 0.01
Selfserving	0.0005	1.58	0.13
Lambda	−0.0051	−4.71	< 0.01
R ²		95.14%	
n		1,576	

Results are based on OLS estimation of Equation (5) on the combined seemingly self-serving and non-self-serving samples after excluding observations with standardized residuals greater than 3 using the Huber M procedure. Standard errors are clustered by firm and by calendar quarter. p-values are two-tailed. All variable definitions are in Panel B of Table 1.

serving attributions even if they lack plausibility, consistent with the “opportunistic behavior” view of capital markets described in Givoly, Hayn, and Katz (2010).²⁶

Sensitivity Analysis

Impact of Industry Leaders on the Calculation of BadQtr

Our current *BadQtr* measure gives equal weight to all firms included in its construction, regardless of their market shares. It is possible that the earnings news of industry leaders may be more influential in assessing the pervasiveness of bad news for the industry and, therefore, industry leaders should receive greater weight in the *BadQtr* measure. We address this concern in two ways. First, we evaluate the extent to which the earnings of the industry leader signals the earnings trend of the whole industry by conducting a correlation analysis. We use the population of Compustat firms to identify industry leaders each year as firms with the largest sales in the industry using the six-digit GICS industry classification. We then construct earnings surprises for all the firm-years with available data and match each firm-year observation with that of its industry leader. Untabulated analysis suggests that the pair-wise correlation of the industry leaders’ earnings news²⁷ with its industry peers is modest (around 5 percent), suggesting that industry leaders’ earnings news may not necessarily weigh more in measuring the pervasiveness of industry-wide bad news.

Second, we reconstruct *BadQtr* variable so that only previously announcing industry leaders are included in the calculation. We define industry leaders as those whose sales for the previous fiscal year are in the top 10 percent for the entire six-digit GICS code and whose market share is at least 10 percent. Untabulated analysis indicates that our inferences remain the same when we use this alternative measure of *BadQtr*.

Controlling for the Preemption Effect of Earlier Arriving Earnings News

The information-transfer literature documents that the stock prices of non-announcing firms react contemporaneously to the earning announcements of earlier announcing firms within their industry, suggesting that the information released by the earlier announcing firms preempts the information yet to be released by the non-announcing firms (Foster 1981; Han and Wild 1990). This preemption effect documented in the information-transfer literature represents a potential confound in interpreting our results if the preemption already experienced due to industry peers’ earlier earnings announcements affects the later stock price response to the non-announcing firm’s own earnings announcement. That is, it is possible that the less severe market penalties of bad news earnings announcements as a function of *BadQtr* reflect the preemption of the bad news due to the earlier arrival of bad news released by industry peers. However, this preemption argument does not explain our finding of greater market rewards to good news earnings announcements as a function of *BadQtr*.

As another sensitivity analysis, we explicitly control for the role of the preemption effect by calculating the abnormal returns of our sample firms on the announcement dates of previously announcing industry peers. We sum up these announcement day returns (*Preemption*) to capture

²⁶ Givoly et al. (2010) argue that capital market scrutiny combined with the linking of manager compensation to stock prices creates pressure for managers to prop up prices by biasing financial reporting. To the extent capital market pressure is greater for firms with higher analyst following and/or institutional ownership, the “opportunistic behavior” argument suggests that greater analyst following and/or institutional ownership may increase managers’ tendency to provide implausible attributions either to mitigate market reactions to negative earnings surprises or to increase market rewards to positive surprises.

²⁷ The correlation is of similar magnitude whether we use the raw value or the sign of earnings surprises.

any preemption that may have occurred when the firm's industry peers release earnings announcements earlier. We augment Equation (1) with the following terms: $SurprisePos \times Preemption$ and $SurpriseNeg \times Preemption$. In untabulated analysis, we find that the coefficient on $SurprisePos \times Preemption$ is insignificant, while the coefficient on $SurpriseNeg \times Preemption$ is significantly positive ($p < 0.01$). More importantly, the coefficients on $SurprisePos \times BadQtr$ and $SurpriseNeg \times BadQtr$ remain significantly positive ($p < 0.01$). This additional analysis confirms that our findings are not attributable to the preemption effects previously documented in the information-transfer literature.

VI. CONCLUSIONS

Using a sample of earnings press releases containing seemingly self-serving attributions issued by a random sample of firms from 1999 to 2005, we examine whether investors respond to seemingly self-serving attributions in earnings press releases based on factors that reflect their plausibility. Conditional on managers' decision to provide seemingly self-serving attributions, we find that defensive attributions are associated with less severe market penalties for bad news earnings announcements when: (1) more of the firm's industry peers also experience earnings disappointments, and (2) the firm's earnings have higher commonality with industry- and market-level earnings. We also find that enhancing attributions are associated with greater market rewards to good news earnings announcements when: (1) more of the firm's industry peers experience earnings disappointments, and (2) the firm's earnings have lower commonality with industry- and market-level earnings. In addition, the market's reliance on the plausibility measures is directionally consistent with these measures' indications for the actual persistence of earnings surprises. Hence, investors' use of the plausibility measures appears justified.

In supplemental analysis, we find that the pattern of the effects of the plausibility factors observed for the seemingly self-serving sample does not extend to the non-self-serving sample, indicating that investors use these measures to assess plausibility only when seemingly self-serving attributions exist. Our sensitivity analyses suggest that our findings are robust to alternative specifications and to controlling for the preemption effect documented in the information-transfer literature.

Our study extends Barton and Mercer's (2005) experimental evidence by providing insight into the information investors appear to rely upon when assessing the plausibility of seemingly self-serving attributions in unstructured market settings where the plausibility of such attributions may not be easily observable. Our finding that investors distinguish the plausibility among seemingly self-serving attributions based on concurrent industry performance and earnings commonality suggests that investors are somewhat sophisticated when interpreting these narrative disclosures.

Our study should be of interest to policymakers who advocate the importance of narrative disclosures "because a numerical presentation and brief accompanying notes alone may be insufficient for an investor to judge the quality of earnings and the likelihood that past performance is indicative of future performance" (Securities and Exchange Commission [SEC] 1987). A potential concern that policymakers face is that managers can use *unregulated* narrative disclosures to manipulate investor perceptions, particularly if investors accept managers' self-serving pronouncements uncritically (Merkl-Davies and Brennan 2007). Our findings mitigate this concern because investors appear to be sophisticated enough to use relevant information to distinguish the plausibility of seemingly self-serving attributions. Apparently, providing self-serving attributions purely to mislead investors may not be an effective strategy.

REFERENCES

- Abrahamson, E., and E. Amir. 1996. The information content of the president's letter to shareholders. *Journal of Business Finance & Accounting* 23 (8): 1157–1182.
- Atiase, R. 1985. Predisclosure information, firm capitalization, and security price behavior around earnings announcements. *Journal of Accounting Research* 23 (1): 21–36.
- Atiase, R., H. Li, S. Supattarakul, and S. Tse. 2005. Market reaction to multiple contemporaneous earnings signals: Earnings announcements and future earnings guidance. *Review of Accounting Studies* 10 (4): 497–525.
- Baginski, S., J. Hassell, and M. Kimbrough. 2004. Why do managers explain their earnings forecasts? *Journal of Accounting Research* 42 (1): 1–29.
- Baginski, S., J. Hassell, and W. Hillison. 2000. Voluntary causal disclosures: Techniques and capital market reaction. *Review of Quantitative Finance and Accounting* 15 (4): 371–389.
- Bamber, L., J. Jiang, K. Petroni, and I. Wang. 2010. Comprehensive income: Who's afraid of performance reporting? *The Accounting Review* 85 (1): 97–126.
- Barton, J., and M. Mercer. 2005. To blame or not to blame: Analysts' reactions to external explanations for poor financial performance. *Journal of Accounting and Economics* 39 (3): 509–533.
- Bettman, J., and B. Weitz. 1983. Attributions in the board room: Causal reasoning in corporate annual reports. *Administrative Science Quarterly* 28 (2): 165–183.
- Bhojraj, C., C. Lee, and D. Oler. 2003. What's my line? A comparison of industry classification schemes for capital market research. *Journal of Accounting Research* 41 (5): 745–774.
- Brown, P., and R. Ball. 1967. Some preliminary findings on the association between the earnings of a firm, its industry, and the economy. *Journal of Accounting Research* 5: 55–77.
- Bryan, S. H. 1997. Incremental information content of required disclosures contained in management discussion and analysis. *The Accounting Review* 72 (2): 285–301.
- Chen, C. 2002. *Robust Regression and Outlier Detection with the ROBUSTREG Procedure*. *Proceedings of the Twenty-Seventh Annual SAS Users Group International Conference*. Cary, NC: SAS Institute Inc.
- Clarkson, P., J. Kao, and G. Richardson. 1999. Evidence that management discussion and analysis (MD&A) is a part of a firm's overall disclosure package. *Contemporary Accounting Research* 16 (1): 111–134.
- Clatworthy, M., and M. Jones. 2003. Financial reporting of good and bad news: Evidence from accounting narratives. *Accounting and Business Research* 33 (3): 171–185.
- Collins, D., and S. P. Kothari. 1989. An analysis of the intertemporal and cross-sectional determinants of the earnings response coefficient. *Journal of Accounting and Economics* 11 (2-3): 143–181.
- Davis, A., J. Piger, and L. Sedor. 2012. Beyond the numbers: Measuring the information content of earnings press release language. *Contemporary Accounting Research* 29 (3): 845–868.
- Easton, P., and M. Zmijewski. 1989. Cross-sectional variation in the stock market response to accounting earnings announcements. *Journal of Accounting and Economics* 11 (2-3): 117–141.
- Financial Accounting Standard Board (FASB). 2001. Improving business reporting: Insights into enhancing voluntary disclosure. Available at: <http://www.fasb.org/brrp/brrp1.pdf>
- Foster, G. 1978. *Financial Statement Analysis*. Englewood Cliffs, NJ: Prentice Hall.
- Foster, G. 1981. Intra-industry information transfers associated with earnings releases. *Journal of Accounting and Economics* 3 (3): 201–232.
- Francis, J., and B. Ke. 2006. Disclosure of fees paid to auditors and the market valuation of earnings surprises. *Review of Accounting Studies* 11 (4): 495–523.
- Francis, J., K. Schipper, and L. Vincent. 2002. Expanded disclosures and the increased usefulness of earnings announcements. *The Accounting Review* 77 (3): 515–546.
- Frazier, K. B., R. W. Ingram, and B. M. Tennyson. 1984. A methodology for the analysis of narrative accounting disclosures. *Journal of Accounting Research* 22 (1): 318–331.
- Freeman, R. 1987. The association between accounting earnings and security returns for large and small firms. *Journal of Accounting and Economics* 9 (2): 195–228.
- Freeman, R., and S. Tse. 1992. A nonlinear model of security price responses to unexpected earnings. *Journal of Accounting Research* 30 (2): 185–209.

- Froot, K. A. 1989. Consistent covariance matrix estimation with cross-sectional dependence and heteroskedasticity in financial data. *Journal of Financial and Quantitative Analysis* 24 (3): 333–355.
- Frost, C. 1997. Disclosure policy choices of UK firms receiving modified audit reports. *Journal of Accounting and Economics* 23 (2): 163–187.
- Givoly, D., C. Hayn, and S. Katz. 2010. Does public ownership of equity improve earnings quality? *The Accounting Review* 85 (1): 195–225.
- Gonedes, N. 1973. Properties of accounting numbers: Models and tests. *Journal of Accounting Research* 11 (2): 212–237.
- Gong, G., L. Li, and L. Zhou. 2013. Earnings non-synchronicity and voluntary disclosure. *Contemporary Accounting Research* (forthcoming).
- Han, J., and J. Wild. 1990. Unexpected earnings and intra-industry information transfers: Further evidence. *Journal of Accounting Research* 28 (1): 211–219.
- Hayn, C. 1995. The information content of losses. *Journal of Accounting and Economics* 20 (2): 125–153.
- Heckman, J. 1979. The sample selection bias as a specification error. *Econometrica* 47 (1): 153–62.
- Hirst, D., L. Koonce, and P. Simko. 1995. Investor reactions to financial analysts' research reports. *Journal of Accounting Research* 33 (2): 335–351.
- Hodge, F., P. Hopkins, and J. Pratt. 2006. Management reporting incentives and classification credibility: The effects of reporting discretion and reputation. *Accounting, Organizations and Society* 31 (7): 623–634.
- Huber, P. 1973. Robust regression: Asymptotics, conjectures and Monte Carlo. *The Annals of Statistics* 1 (5): 799–821.
- Hutton, A., G. Miller, and D. Skinner. 2003. The role of supplementary statements with management earnings forecasts. *Journal of Accounting Research* 41 (5): 867–890.
- Ingram, R., and K. Frazier. 1983. Narrative disclosures in annual reports. *Journal of Business Research* 11 (1): 49–60.
- Kelley, H. 1971. Attribution in social interaction. In *Attribution: Perceiving the Causes of Behavior*, edited by E. E. Jones, D. E. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, and B. Weiner, 1–26. Morristown, NJ: General Learning Press.
- Koch, A. S. 2002. *Financial Distress and the Credibility of Management Earnings Forecasts*. Working paper, Carnegie Mellon.
- Koonce, L., and M. Mercer. 2005. Using psychology theories in archival financial accounting research. *Journal of Accounting Literature* 24: 175–214.
- Koonce, L., M. Williamson, and J. Winchel. 2010. Benefit of doubt in financial reporting. *The Accounting Review* 85 (3): 979–1000.
- Koonce, L., N. Seybert, and J. Smith. 2011. Causal reasoning in financial reporting and voluntary disclosure. *Accounting, Organizations and Society* 36 (4–5): 209–225.
- Lennox, C., J. Francis, and Z. Wang. 2012. Selection models in accounting research. *The Accounting Review* 87 (2): 589–616.
- Lim, S., B. Ro, and Y. Pyo. 2001. Analysts' earnings forecast revision around industry member firms' earnings announcement. *Advances in Accounting* 18: 149–168.
- Magee, R. 1974. Industry-wide commonalities in earnings. *Journal of Accounting Research* 12 (2): 270–287.
- Mercer, M. 2005. How do investors assess the credibility of management disclosures? *Accounting Horizons* 18 (3): 185–196.
- Merkel-Davies, D., and N. Brennan. 2007. Discretionary disclosure strategies in corporate narratives: Incremental information or impression management? *Journal of Accounting Literature* 26: 116–194.
- Merkley, K., L. Bamber, and T. Christensen. 2013. Detailed management earnings forecasts: Do analysts listen? *Review of Accounting Studies* 18 (2): 479–521.
- Miller, D., and M. Ross. 1975. Self-serving biases in attribution of causality: Fact or fiction. *Psychological Bulletin* 82 (2): 213–225.
- Pownall, G., C. Wasley, and G. Waymire. 1993. The stock price effects of alternative types of management earnings forecasts. *The Accounting Review* 68 (4): 896–912.

Ramnath, S. 2002. Investor and analyst reactions to earnings announcements of related firms: An empirical analysis. *Journal of Accounting Research* 40 (5): 1275–1376.

Salancik, G., and J. Meindl. 1984. Corporate attributions as strategic illusions of management control. *Administrative Science Quarterly* 29 (2): 238–254.

Schrand, C., and B. Walther. 2000. Strategic benchmarks in earnings announcements: The selective disclosure of prior-period earnings components. *The Accounting Review* 75 (2): 151–177.

Securities and Exchange Commission (SEC). 1987. *Concept Release on Management’s Discussion and Analysis of Financial Condition and Results of Operations*. Securities Act Release No. 6711. Washington, DC: Government Printing Office.

Skinner, D. 1994. Why firms voluntarily disclose bad news. *Journal of Accounting Research* 32 (1): 38–60.

Smith, M., and R. Taffler. 1992. Readability and understandability: Different measures of the textual complexity of accounting narrative. *Accounting, Auditing and Accountability Journal* 5 (4): 84–98.

Smith, M., and R. Taffler. 2000. The chairman’s statement—A content analysis of discretionary narrative disclosures. *Accounting, Auditing and Accountability Journal* 13 (5): 624–647.

Snyder, M., W. Stephan, and D. Rosenfeld. 1978. Attributional egotism. In *New Directions in Attribution Research*, edited by J. H. Harvey, W. J. Ickes, and R. F. Kidd, 2, 91–117. Hillsdale, NJ: Lawrence Erlbaum.

Staw, B. 1980. Rationality and justification in organizational life. In *Research in Organizational Behavior*, edited by B. M. Staw, and L. Larry, Cummings, 2, 45–80. Greenwich, CT: JAI Press.

Staw, B., P. McKechnie, and S. Puffer. 1983. The justification of organizational performance. *Administrative Science Quarterly* 28 (4): 582–600.

Tennyson, B., R. Ingram, and M. Dugan. 1990. Assessing the information content of narrative disclosures in explaining bankruptcy. *Journal of Business Finance & Accounting* 17 (3): 391–410.

Tse, S., and J. Tucker. 2009. Within-industry timing of earnings warnings: Do managers herd? *Review of Accounting Studies* 15 (4): 879–914.

Wooldridge, J. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: The MIT Press.

APPENDIX A
Classification Scheme of Attributions
Categories of External and Internal Attributions in Earnings Press Releases

Exhibit 1 as in Baginski, Hassell, and Kimbrough (2004)

External Causes	Internal Causes
General Economic/Environmental Issues	Product/Services Issues/Actions
Recession/inflation	Changes in product prices
Dollar weakness/strength	Changes in product mix
Foreign currency fluctuation	Advertising/marketing
Input cost changes—increasing/decreasing costs	New products/processes/production
Change in market for product	
General loss/gain of customers	
Weather/catastrophe	
Order backlogs	
Governmental/Third Party Issues	Organizational Issues/Actions
Tax law/other law changes	Management techniques/strategies/plans/repositioning
SEC actions/regulatory actions	Changes in management personnel
Expropriation by foreign governments	Cost cutting/savings
Lawsuits/legal actions	Asset write downs

Competition action/issues	Going public
Involuntary accounting changes	Selling/buying stocks
	Merger/acquisition/disposal of a business segment
	Investment in plant assets
	Voluntary accounting changes

Examples of Seemingly Self-Serving Attributions

Excerpts from Earnings Press Releases

Defensive Attributions

- “UGI Corp. (UGI) attributed lower results to warm weather since September 1999 and higher propane products costs.” (UGI Corp., July 26, 2000)
- “The majority of the revenue decline in Europe can be attributed to currency changes in the Euro and British Pound.” (Cascade Corporation, April 13, 2006)

Enhancing Attributions

- Commenting on the results for the quarter, Chairman, President and Chief Executive Officer Warren Bryant said, “Achieving a 75 percent increase in net income on a total sales increase of 2 percent reflects the progress we have made on our initiatives.” (Longs Drug Stores Corporation, November 19, 2003)
- J. Patrick Gallagher, Jr., President and Chief Executive Officer, stated, “The Company’s continued efforts on cost containment and operational efficiencies . . . contributed positively to our first quarter results.” (Arthur J. Gallagher & Co., April 19, 1999)

Taking the Pulse of the Real Economy Using Financial Statement Analysis: Implications for Macro Forecasting and Stock Valuation

Yaniv Konchitchki
Panos N. Patatoukas
University of California, Berkeley

ABSTRACT: In this study, we hypothesize and find that financial statement analysis of firm profitability drivers applied at the aggregate level yields timely insights that are relevant for forecasting real economic activity. We first show that focusing on the 100 largest firms offers a cost-effective way to extract information embedded in accounting profitability data of the entire stock market portfolio. We then show that accounting profitability data aggregated across the 100 largest firms have predictive content for subsequent real Gross Domestic Product (GDP) growth. We also show that stock market returns have predictive content for future real GDP growth, while their predictive power varies with the length of the measurement window with annual stock market returns being the most powerful. Importantly, we find that the predictive content of our indices of aggregate accounting profitability drivers is incremental to that of annual stock market returns. An in-depth investigation of consensus survey forecasts shows that professional macro forecasters revise their expectations of real economic activity in the direction of the predictive content of aggregate accounting profitability drivers and stock market returns. Although macro forecasters are fully attuned to stock market return data, their forecasts of real GDP growth can be improved in a statistically and economically significant way using our indices of aggregate accounting profitability drivers. Our findings suggest that professional macro forecasters and stock market investors do not fully impound the predictive content of aggregate accounting profitability drivers when forecasting real economic activity. In additional analysis, we examine the association between stock market returns and the portion of subsequent real GDP growth that is predictable based on our indices of aggregate accounting profitability drivers but that is not anticipated by stock market investors. We find that this portion is positively related to stock market returns, suggesting that the macro predictive content of aggregate accounting profitability drivers is relevant for stock valuation. Overall, our study brings

We thank John Harry Evans III (senior editor), Kenneth A. Merchant (editor), and two anonymous reviewers for their comments and suggestions. We also thank Severin Borenstein, Sunil Dutta, Reut Laufer, Topseht Nonam, George Patatoukas, Mary Psyllaki, Andy Rose, and seminar participants at the University of California, Berkeley for helpful comments and suggestions. We gratefully acknowledge financial support from the Hellman Fellows Program and the Center for Financial Reporting and Management at the University of California, Berkeley, Haas School of Business.

Editor's note: Accepted by Kenneth A. Merchant.

Submitted: April 2012
Accepted: July 2013
Published Online: October 2013

financial statement analysis to the forefront as an incrementally useful tool for gauging the prospects of the real economy that should be of interest to academics and practitioners.

Keywords: *accounting; financial statement analysis; macro forecasting; stock valuation; macroeconomics.*

JEL Classification: *E01; E32; E60; M41.*

Data Availability: *Data are available from public sources indicated in the text.*

I. INTRODUCTION

How will the macroeconomy fare in the future? The answer to this question is the “holy grail” for macroeconomists and a wide range of decision makers. In this study, we investigate the usefulness of financial statement analysis based on accounting profitability data from individual firms for taking the pulse of the U.S. real economy.

Using financial statement analysis to forecast economic activities at the firm level has long been a topic of academic research in accounting (Ou and Penman 1989; Penman 1992; Lev and Thiagarajan 1993; Abarbanell and Bushee 1998; Nissim and Penman 2001; Konchitchki 2011; Patatoukas 2012). A key building block of financial statement analysis conducted at the firm level is DuPont profitability analysis, which decomposes a firm’s accounting rate of return on net operating assets (*RNOA*)—the primary measure of a firm’s core operating performance—into asset turnover and profit margin. Prior studies provide evidence that changes in *RNOA* and its drivers are useful for forecasting economic activity at the firm level (Fairfield and Yohn 2001; Nissim and Penman 2001; Soliman 2008), yet little is known about the usefulness of financial statement analysis for understanding the prospects of the overall economy. Our study helps fill this gap.

Publicly traded firms in the U.S. are required to report financial statements on a quarterly basis. These quarterly reports provide timely information about each listed firm’s underlying economic activities. Because listed firms represent a large part of the U.S. economy, changes in their respective economic activities can be informative about shifts in overall economic activity (Fama 1981). We therefore conjecture that if the financial reporting system captures changes in economic activity at the firm level on a timely basis, then financial statement analysis of firm profitability drivers at the aggregate level can provide timely clues as to the prospects of the U.S. real economy.

To test the above conjecture, we collect income statement and balance sheet data for a sample of publicly traded U.S. firms and construct indices of aggregate changes in profitability and DuPont profitability drivers using value-weighted cross-sectional averages. To mitigate the costs of collecting and aggregating financial statement data imposed on macro forecasters, we restrict our sample to the 100 largest firms. Over our 1981:Q3 to 2011:Q3 sample period, we document that the 100 largest firms represent the vast majority of the stock market in terms of market capitalization. Hence, understanding fluctuations in their performance can offer insights into the performance of the universe of listed U.S. firms. Indeed, we find that aggregate changes in profitability and profitability drivers for the 100 largest firms are almost perfectly correlated with the corresponding changes for *all* listed U.S. firms. Focusing on the 100 largest firms therefore offers a cost-effective way to extract information embedded in the accounting profitability data of the entire stock market portfolio.

Consistent with our conjecture, we find that financial statement analysis of firm profitability drivers at the aggregate level is useful for macro forecasting. We document a significantly positive association between our index of aggregate changes in accounting profitability ($\Delta RNOA$) and subsequent real GDP growth. The DuPont profitability analysis reveals that aggregate changes in asset turnover and profit margins are leading indicators of real economic activity. However, over

our sample period, the predictive ability of profit margins swamps that of asset turnover, with the predictive ability of $\Delta RNOA$ driven primarily by aggregate changes in profit margins. After decomposing profit margins into operating margins, given by the ratio of operating income before depreciation to sales, and the ratio of depreciation to sales, a proxy for tangible capital intensity (Lev 1983; Cheng 2005), we find that aggregate changes in both components are significantly positively associated with subsequent real GDP growth. Taken together, aggregate changes in accounting profitability drivers anticipate 26 percent of the time-series variation in subsequent real GDP growth.

To evaluate the incremental usefulness of aggregate accounting profitability drivers for macro forecasting, we focus on the stock market. Consistent with rational expectations asset pricing models (Fama 1981; Fischer and Merton 1984; Barro 1990; Fama 1990), we find that stock market returns contain leading information about overall economic activity. Specifically, stock market returns positively predict subsequent real GDP growth, while their predictive power varies with the length of the measurement window with annual stock market returns being the most powerful. Importantly, we show that the predictive content of aggregate accounting profitability drivers is not subsumed by that of annual stock market returns, suggesting that financial statement analysis of firm profitability drivers at the aggregate level is *incrementally useful* for macro forecasting. Indeed, the use of aggregate accounting profitability data leads to significant improvements in terms of explanatory power with the adjusted R^2 rising from 20 percent when annual stock market returns are included as stand-alone predictors of subsequent real GDP growth, to 36 percent when annual stock market returns are included together with aggregate changes in accounting profitability drivers.

An in-depth investigation of consensus forecasts from the Survey of Professional Forecasters (SPF)—the longest and most highly regarded quarterly survey of macro forecasts in the U.S.—reveals that macro forecasters revise their expectations of real economic activity in the direction of the predictive content of aggregate accounting profitability drivers and stock market returns. However, macro forecasters are not fully attuned to aggregate accounting profitability drivers and thus their real GDP growth forecast errors are predictable based on lagged accounting profitability data. Indeed, we find that macro forecasters' projections of real GDP growth can be improved in a statistically and economically significant way using our indices of aggregate accounting profitability drivers. In contrast, real GDP growth forecast errors are not predictable based on lagged stock market returns, suggesting that macro forecasters fully impound the predictive content of stock market returns for the prospects of the real economy. Our evidence is consistent with the fact that stock market return data are known to have predictive ability for the real economy (Fama 1981) and are readily available to macro forecasters, while aggregate accounting profitability data are not.

Given our evidence that stock market returns do not subsume the macro predictive content of aggregate accounting profitability drivers, it follows that investors' projections of real economic activity embedded in stock market prices can also be improved based on aggregate accounting profitability data. In additional analysis, we examine the association between stock market returns and the portion of real GDP growth that is predictable based on our indices of aggregate accounting profitability drivers but that is not anticipated by stock market investors. We find that this portion is positively related to stock market returns, suggesting that the predictive content of aggregate accounting profitability drivers for real economic activity is relevant for stock valuation.

Our study brings financial statement analysis of firm profitability drivers to the forefront as an incrementally useful tool for macro forecasting. Our evidence shows that aggregate accounting profitability drivers embed timely information about the prospects of the U.S. real economy. Accordingly, our study can lead to improvements in macro forecasting using accounting profitability data from individual firms. Importantly, from a practical implementation standpoint,

our evidence on the informativeness of the 100 largest firms offers academics and practitioners a cost-effective way to extract information embedded in accounting profitability data of the entire stock market portfolio. Although our study focuses on the U.S., the analysis can be extended to the international context.

We contribute to several streams of literature. First, our findings complement the evidence in Konchitchki and Patatoukas (2013) that aggregate accounting earnings growth is a leading indicator of the nominal economy. Our study extends the evidence in Konchitchki and Patatoukas (2013) along many dimensions by combining income statement and balance sheet data in a cost-effective way and by investigating the relevance of financial statement analysis of firm profitability drivers for forecasting real GDP growth and stock valuation. Second, we contribute to macroeconomics research by identifying an incrementally significant link between aggregate accounting profitability drivers and subsequent real GDP growth. We further contribute to the macro forecasting literature by showing that professional macro forecasters can improve real GDP growth projections by incorporating accounting profitability data from large publicly traded firms in a cost-effective way. Finally, we contribute to capital markets research in accounting and finance by providing evidence that the predictive content of aggregate accounting profitability drivers for real GDP growth is relevant for stock valuation.

Viewed as a whole, our findings have implications for a wide range of decision makers who are interested in macro forecasting. In line with recent calls to make accounting research more useful (Moehrle et al. 2009; Merchant 2012), we believe that our study has the potential to make a contribution to the advancement of macro forecasting practice. Our findings should also be of interest to accounting standard-setters. We identify an important group of users of accounting information, namely, those parties interested in gauging macroeconomic activity, including professional macro forecasters and macroeconomists staffing the Federal Reserve and the White House. This group is often overlooked by the Financial Accounting Standards Board as users of accounting information.

Section II next provides background and motivates our research questions. Section III describes our research design. Section IV examines the *usefulness* of financial statement analysis and stock market returns for macro forecasting. Section V investigates the *use* of financial statement analysis and stock market returns by macro forecasters. Section VI concludes.

II. BACKGROUND AND RESEARCH QUESTIONS

A long line of accounting research investigates the usefulness of financial statement analysis for forecasting future changes in firm fundamentals (Ou and Penman 1989; Penman 1992; Lev and Thiagarajan 1993; Abarbanell and Bushee 1998; Nissim and Penman 2001; Konchitchki 2011; Patatoukas 2012). Researchers typically use the ratio of operating income after depreciation to net operating assets, referred to as the return on net operating assets (*RNOA*), as a comprehensive measure of overall firm performance. Operating income is defined as sales minus cost of goods sold, selling, general, and administrative expense, and depreciation expense. Net operating assets are defined as operating assets, which are total assets minus cash and short-term investments, minus operating liabilities, which are total liabilities minus long- and short-term debt. Both operating income and net operating assets abstract away from the effects of financial leverage, so *RNOA* provides an unlevered measure of firm operating performance.

The return on net operating assets can be decomposed into two profitability drivers as follows:

$$RNOA = \frac{Sales}{Net\ Operating\ Assets} \times \frac{Operating\ Income\ After\ Depreciation}{Sales}.$$
 (1)

The first driver, the ratio of sales to net operating assets, captures asset turnover (*ATO*). Asset

turnover measures a firm's ability to generate revenues from its assets. The second driver, the ratio of operating income after depreciation to sales, captures profit margin (*PM*). Profit margin measures a firm's ability to control the costs incurred to generate revenues and to charge premium prices. This decomposition of *RNOA* is commonly referred to as DuPont profitability analysis.

Textbooks such as Penman (2001) advocate the use of DuPont profitability analysis as a building block of financial statement analysis. Also academic research provides empirical evidence that changes in *RNOA* and its drivers are useful for forecasting firm fundamentals (Fairfield and Yohn 2001; Nissim and Penman 2001; Soliman 2008). Yet despite the prominence of financial statement analysis of firm profitability for forecasting economic activity at the firm level, there is a dearth of evidence on its usefulness for forecasting *overall* economic activity.

Publicly traded firms in the U.S. are required to report financial statements on a quarterly basis. These reports provide timely forward-looking information about each listed firm's underlying economic activities. Because listed firms represent a large part of the U.S. economy, changes in their respective economic activities can be informative about shifts in overall economic activity (Fama 1981). Accordingly, we conjecture that if the financial reporting system captures changes in economic activity at the firm level on a timely basis, then financial statement analysis of firm profitability drivers applied at the aggregate level can offer a window to timely macroeconomic clues.

Based on the above discussion, our first research question is whether aggregate changes in profitability and profitability drivers are useful for forecasting growth in overall economic activity. We use real GDP growth as our measure of economic growth. Real GDP, featured in the National Income and Product Accounts (NIPA) prepared by the Bureau of Economic Analysis (BEA), measures the inflation-adjusted value added at each stage of the production process of goods and services produced in the U.S. economy (BEA 2007). Although recent research focuses on the link between accounting data and nominal economic activity (Basu, Markov, and Shivakumar 2010; Cready and Gurun 2010; Shivakumar 2010; Konchitchki 2011; Kothari, Shivakumar, and Urcan 2013; Konchitchki and Patatoukas 2013; Patatoukas 2013), we focus on real GDP growth because we want to abstract from the link between accounting data and inflation.

We focus on the usefulness for macro forecasting of aggregate changes in *RNOA* and its drivers rather than their levels because our objective is to forecast growth in economic activity rather than the level of economic activity. Compared to other accounting rates of return, such as return on equity or return on assets, *RNOA* offers a more appealing means for gauging economic activity at the aggregate level. This is because *RNOA* is based on unlevered financial statements and offers a measure of economic activity at the enterprise level that lies at the center of value creation for equity and debt capital providers, paralleling GDP as a measure of value added at the aggregate level.

In the vein of rational expectations asset pricing models, one could envisage stock market investors as a group of macro forecasters (Fama 1981; Fischer and Merton 1984; Barro 1990; Fama 1990). Given that stock market prices are related to investors' expectations of future overall economic activity, our second research question is whether aggregate accounting profitability drivers are *incrementally useful* for macro forecasting. To directly address this research question, we begin by testing whether stock market returns measured over different windows anticipate future real GDP growth. We then investigate whether aggregate accounting profitability drivers embed forward-looking information for real GDP growth that is not subsumed by stock market returns.

Our next set of research questions examines whether professional macro forecasters use forward-looking information embedded in aggregate accounting profitability drivers and stock market returns when forecasting real economic activity. We obtain consensus forecasts of real GDP growth from the SPF—the oldest and most highly regarded quarterly survey of macro forecasts in

the U.S.¹ The SPF forecasts are produced by a broad-based group of professionals affiliated with financial institutions, including Bank of America Merrill Lynch, Barclays Capital, Credit Suisse, Deutsche Bank, Goldman Sachs, JPMorgan Chase, Moody's, and Wells Fargo, as well as nonfinancial institutions, including academic institutions, the government, trade associations, and labor unions. The SPF panelists have incentives to provide accurate forecasts because their reputation with the Federal Reserve is at stake and they report to the survey the same forecasts that they sell to market participants (Baghestani and Kianian 1993).²

The SPF consensus forecasts are used as benchmarks for the assumptions underlying the U.S. Federal Budget. The SPF forecasts are also central for monetary policy because they are used by the research staff of the Board of Governors of the Federal Reserve when preparing the "Greenbook" before each meeting of the Federal Open Market Committee. To gain more insights into whether the SPF panelists are sufficiently incentivized, we compare GDP growth projections from the SPF with those from the Greenbooks. The Federal Reserve Board of Governors is incentivized to produce accurate projections of the U.S. economy because its Greenbook projections are used, for example, when formulating monetary policy. Unfortunately, the Greenbook projections become available to the public only after a five-year embargo. Because of the five-year embargo macro forecasters cannot use these projections in real time and so they are not within the feasible information set of the SPF panelists. Consistent with Sims (2002), we find that the Greenbook projections are indistinguishably different from the SPF consensus forecasts, which indicates that the incentives of the SPF panelists are aligned with the incentives of those with the strongest incentives to produce accurate forecasts.

Our first objective is to test whether professional macro forecasters respond to any incremental predictive content embedded in aggregate accounting profitability drivers. Toward this end, we examine the association of revisions in SPF consensus forecasts of future real GDP growth with aggregate accounting profitability drivers and stock market returns. Our second related objective is to test whether professional macro forecasters fully impound leading information embedded in aggregate accounting profitability drivers and stock market returns when forecasting real GDP

¹ The SPF began in 1968 and was originally conducted by the National Bureau of Economic Research (NBER) and the American Statistical Association (ASA). The Federal Reserve Bank of Philadelphia took responsibility of the SPF in 1990. We note that the Livingston Survey, started in 1946 by the late financial columnist Joseph Livingston, is the oldest semi-annual survey of macro forecasts in the U.S. The Federal Reserve Bank of Philadelphia also took over responsibility of the Livingston Survey in 1990. There are two limitations to the Livingston Survey. First, it provides only six-month forecasts twice a year and thus is not as timely as the quarterly SPF. Second, it is characterized by inconsistencies and *ad hoc* adjustments prior to 1990 (see Livingston Survey Documentation available on the website of the Federal Reserve Bank of Philadelphia, available at: <http://www.phil.frb.org/research-and-data/real-time-center/livingston-survey>). Another survey of professional forecasters is the Blue Chip Survey. Unlike the SPF and the Livingston Survey, the Blue Chip Survey is not publicly available; it is available for purchase from Aspen Publishers.

² The SPF consensus forecasts of subsequent GDP growth are known to outperform benchmark projections obtained from a naïve random walk and from more sophisticated time-series models (Zarnowitz and Braun 1993; Fildes and Stekler 2002; Stark 2010; Wieland and Wolters 2011). Despite the superior accuracy of the SPF consensus forecasts relative to time-series benchmarks, however, there is evidence that SPF forecasters have made errors when the U.S. economy was subject to perturbations. Specifically, macro forecasters have over-predicted GDP growth in slowdowns and recessions, and under-predicted growth in recoveries and booms (Zarnowitz and Braun 1993; Schuh 2001; Fildes and Stekler 2002; Stock and Watson 2003). In untabulated analysis, using the business-cycle classification dates from the NBER we find that real GDP growth forecast errors tend to be negative during recessions and positive during expansions, suggesting that macro forecasters tend to be negatively surprised during recessions and positively surprised during expansions. One could argue, however, that macro forecasting is more difficult during periods characterized by a high degree of economic turbulence (McNees 1992; Croushore 2002; Krane 2003). A point of general agreement is that, given the importance and widespread use of GDP growth projections, the rewards from even a slight improvement in forecast accuracy are clearly large.

growth. To do so, we search for predictability in real GDP growth forecast errors based on aggregate accounting profitability drivers and stock market returns.

In the absence of evidence that financial statement analysis applied at the aggregate level is useful for forecasting real economic activity, professional macro forecasters may simply be unaware of potential “net” benefits from collecting and aggregating income statement and balance sheet data. To the extent that macro forecasters do not fully incorporate relevant accounting profitability data in their projections, real GDP growth forecast errors could be predictable based on lagged accounting information. In contrast, we have reasons to believe that macro forecasters are attuned to the idea that stock market returns are useful for macro forecasting. This is because stock market return data are known to have predictive ability for real economic activity (Fama 1981) and are readily available to macro forecasters.

Overall, our study is positioned to provide new evidence on both the *usefulness* and the *use* of financial statement analysis and stock market returns for forecasting real economic activity. We do so by addressing the following research questions: (1) Do aggregate accounting profitability drivers include forward-looking information about real GDP growth? (2) Is there macro predictive content in aggregate accounting profitability drivers incremental to that in stock market returns? (3) Are macro forecasters’ revisions in expectations related to aggregate accounting profitability drivers and stock market returns? (4) Do macro forecasters fully impound any macro predictive content in aggregate accounting profitability drivers and stock market returns when forecasting real GDP growth? (5) What are the implications for stock valuation of any macro predictive content of aggregate accounting profitability drivers? Answers to these questions should be of interest to academics and practitioners.

III. RESEARCH DESIGN

We collect a sample of U.S.-listed firms with income statement and balance sheet data on the Compustat quarterly file. To align fiscal quarters with calendar quarters, we use firms with calendar fiscal quarter-end dates of March, June, September, and December. For each firm-quarter, we measure profitability (*RNOA*) and the DuPont profitability drivers, asset turnover (*ATO*), and profit margin (*PM*). Profit margin, the ratio of operating income after depreciation to sales, can be further decomposed into the ratio of operating income before depreciation to sales, or operating margin (*OM*), and the ratio of depreciation to sales (*DEP*), a proxy for tangible capital intensity (Lev 1983; Cheng 2005). To obtain annualized profitability ratios, we multiply all income statement variables by 4. To seasonally adjust accounting data, we use year-over-year changes in quarterly profitability ratios, indicated by Δ . To mitigate the influence of outliers, we exclude observations falling in the top or bottom 1 percent of each quarterly cross section of the levels or changes in *RNOA* and its drivers.

Our research design is geared towards the timing of the SPF. As Figure 1, Panel A illustrates, the survey questionnaires are sent to the SPF panelists by the end of the first month after the quarter ends. Accordingly, we restrict our sample to firms with accounting data available by the end of the first month, month t , after quarter q ends. Figure 1, Panel B illustrates the timing for the 2011 third-quarter SPF, which is the most recent survey in our sample. The questionnaires were sent to macro forecasters by the end of July 2011, so the accounting data released by that time were within the feasible information set of the SPF panelists. We note that the SPF panelists are required to submit the survey questionnaires by the middle of the second month after the end of each quarter, which would be the middle of August for the third-quarter SPF. Effectively, our research design allows macro forecasters time to collect and aggregate income statement and balance sheet data.

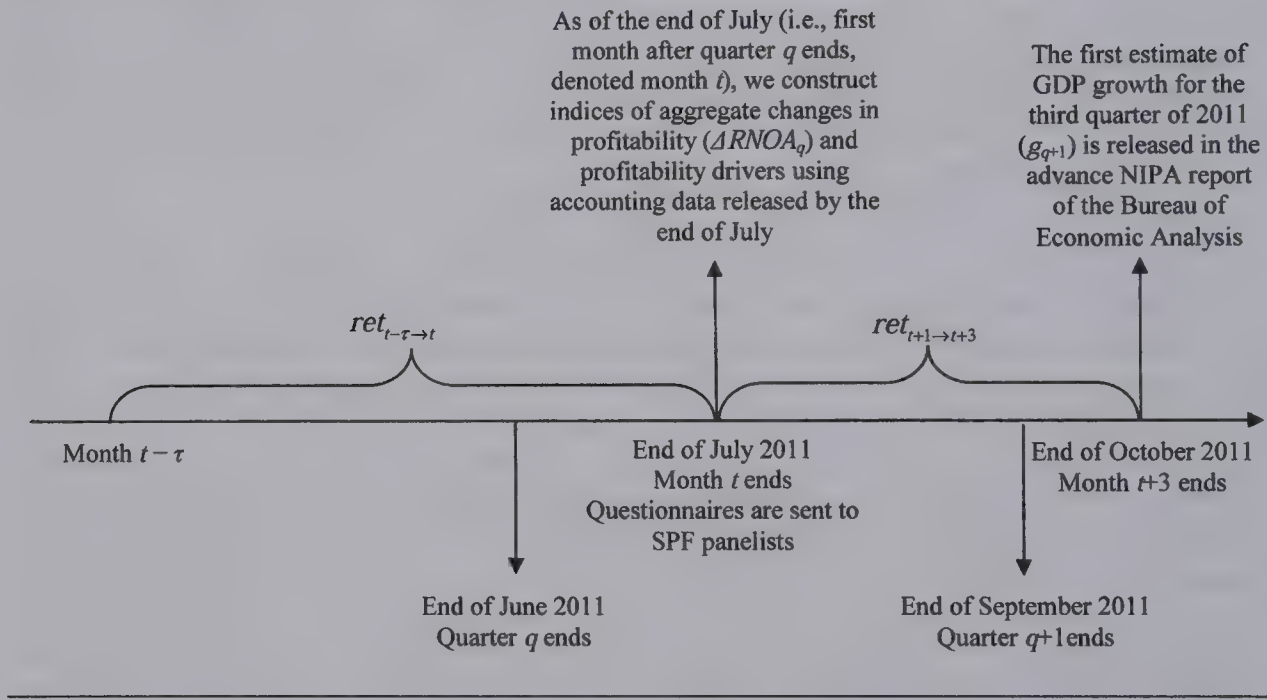
We acknowledge that collecting and aggregating data from corporate financial reports imposes non-trivial acquisition and processing costs on macro forecasters. To mitigate such costs, we further

FIGURE 1
Timeline of Research Design

Panel A: The Timing of the SPF

Name of SPF	Date Questionnaires Sent to SPF Panelists	Submission Deadline for SPF Questionnaires
First Quarter	End of January	Middle of February
Second Quarter	End of April	Middle of May
Third Quarter	End of July	Middle of August
Fourth Quarter	End of October	Middle of November

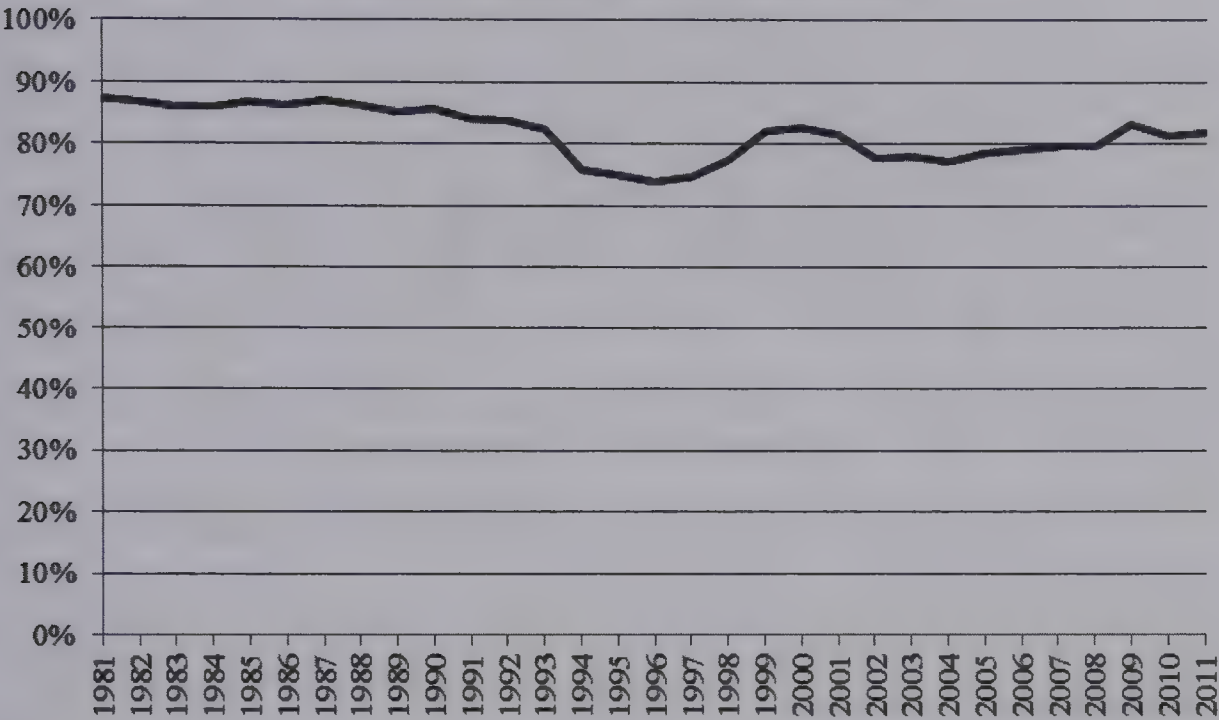
Panel B: Example for 2011 Third-Quarter SPF



Panel A summarizes the timing of the SPF. The questionnaires are sent to the SPF panelists by the end of the first month, month t , after quarter q ends. The accounting data released by the end of month t as well as stock market returns realized by the end of month t are within the feasible information set of SPF panelists when forming their expectations.

Panel B illustrates the timeline of our research design. Panel B also illustrates the measurement of buy-and-hold stock market returns (ret) over the $\tau = 3, 6, 12$, and 24 months leading to the end of month t as well as over the 3 months after the end of month t .

FIGURE 2
Relative Importance of 100 Largest Firms



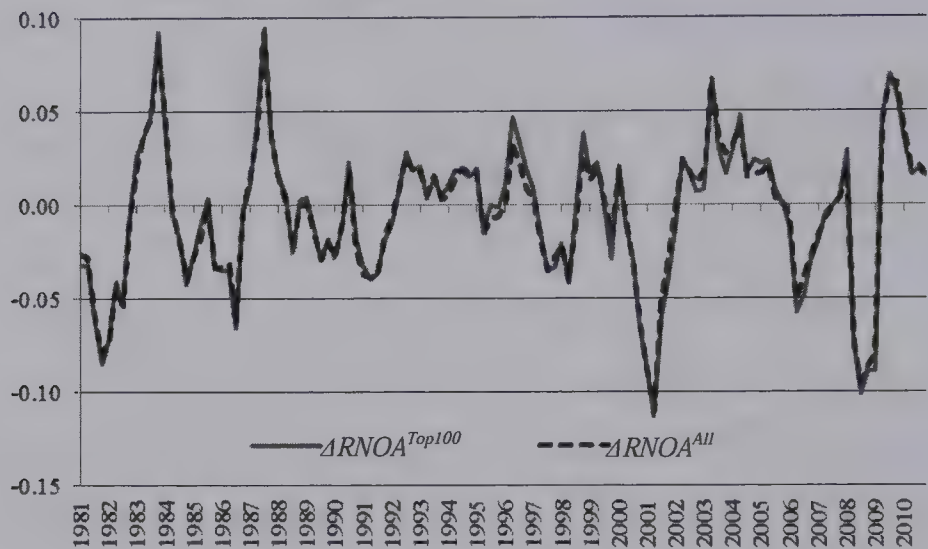
This figure presents the time-series of the sum of the market capitalization of the 100 largest firms as a percentage of the sum of the market capitalization of all firms with accounting data released by the end of the first month, month t , after quarter q ends. The sample period is from 1981:Q3 to 2011:Q3.

restrict our sample to the 100 largest firms in terms of market capitalization measured at the beginning of each quarter. Figure 2 reports the time-series of the total market capitalization of the 100 largest firms as a fraction of the total market capitalization of all firms with available accounting data within one month after the quarter ends. Over our sample period, the 100 largest firms account for 82 percent of the total market capitalization of all firms, and thus understanding fluctuations in their performance could offer considerable insights into the performance of *all* publicly traded firms. Indeed, Figure 3 shows that profitability changes for the 100 largest firms move in lockstep with profitability changes for all firms: the correlation between the two series is 0.99, a finding that also holds for profitability drivers. Clearly, the evidence in Figures 2 and 3 suggests that focusing on the 100 largest firms offers a cost-effective way to extract information embedded in accounting profitability data of the entire stock market portfolio.

Our final sample includes 12,000 firm-quarter observations, representing the 100 largest firms per quarter with accounting data released within one month after the quarter ends, over 120 quarters from 1981:Q3 to 2011:Q3. The composition of the 100 largest firms can change from one quarter to another depending on data availability and changes in the cross-sectional distribution of market capitalization. The sample period begins in 1981:Q3 because from that point onward the SPF's schedule has been consistent over time.³ The sample period ends in 2011:Q3 because this is the last quarter for which we can obtain data necessary for our analyses. We construct aggregate indices of

³ Our inferences are similar when we repeat our analysis for the post-1990 period after the Federal Reserve Bank of Philadelphia took responsibility of the SPF from the NBER/ASA.

FIGURE 3
Aggregate Changes in Accounting Profitability



This figure presents the time-series of aggregate changes in the rate of return on net operating assets. We construct the aggregate time-series using value-weighted cross-sectional averages based on two samples: (1) the sample of 100 largest firms (in terms of lagged market capitalization) with accounting data released by the end of the first month, month t , after quarter q ends ($\Delta RNOA^{Top100}$, denoted for brevity as $\Delta RNOA$ throughout the rest of the manuscript); and (2) the sample of all firms with accounting data released by the end of the first month, month t , after quarter q ends ($\Delta RNOA^{All}$). The sample period is from 1981:Q3 to 2011:Q3.

levels and changes in $RNOA$ and its drivers based on value-weighted cross-sectional averages and use market capitalization measured at the beginning of each quarter to weight observations.⁴

To evaluate the incremental usefulness of our indices of accounting profitability drivers for macro forecasting, we measure buy-and-hold stock market returns ($ret_{t-\tau \rightarrow t}$) over the $\tau = 3, 6, 12$, and 24 months leading to the end of the first month, month t , after quarter q ends. We proxy for the stock market portfolio using two well-accepted indices, namely, the CRSP value-weighted index including distributions, and the S&P 500 index.

We obtain data on subsequent real GDP growth, denoted g_{q+1} , from the BEA’s first, also known as the advance, NIPA report published by the end of the first month after the quarter ends.⁵ The advance GDP growth data are available from the Real-Time Data Set for Macroeconomists of the Federal Reserve Bank of Philadelphia. Figure 1, Panel B, illustrates that the advance realization of GDP growth for the third quarter of 2011 was released by the end of October 2011. The advance realization of GDP growth is unavailable for the third quarter of 1995 from the Real-Time Data Set

⁴ Our inferences are not sensitive to whether we construct the aggregate indices using value- or equal-weighted averages. As an additional sensitivity test, we construct the aggregate series of accounting profitability ratios by dividing the cross-sectional sum of each ratio’s numerator by the cross-sectional sum of the ratio’s denominator. Consistent with Kothari, Lewellen, and Warner (2006), we observe that the value-weighted series are highly correlated with the series constructed based on cross-sectional sums. Our inferences are unchanged using aggregate series based on cross-sectional sums.

⁵ We focus on the advance realization of GDP growth because NIPA revisions tend to be small and most of the information about the state of the economy is known over the period leading to the BEA’s advance NIPA report (Fixler and Grimm 2005; Lahiri and Wang 2006; Landefeld, Seskin, and Fraumeni 2008). The advance and final realizations of GDP growth have a correlation coefficient of 0.96 and our inferences are not sensitive to this choice.

for Macroeconomists due to a government shutdown, so our sample includes 120 quarters from 1981:Q3 to 2011:Q3. As explained in Section II, we focus our efforts on forecasting real GDP growth because we want to abstract from the link between accounting data and inflation.

We obtain the mean SPF consensus forecast of quarter $q+1$ real GDP growth, denoted $E_q[g_{q+1}]$, from the Federal Reserve Bank of Philadelphia. We focus on the quarter $q+1$ forecasts because professional macro forecasters focus on the one-quarter-ahead forecast horizon (Stark 2010). We focus on the SPF consensus forecasts and not on individual forecasters' projections because none of the individual forecasters is better than the average (Zarnowitz and Braun 1993; Graham 1996; Croushore 2011). We note that the distribution of individual quarterly GDP growth forecasts is fairly symmetric, so our results are insensitive as to whether we use mean or median SPF consensus forecasts. Over our sample period there are, on average, 35 individual forecasters or forecasting groups per survey, for a total of 4,224 responses.

IV. THE INCREMENTAL USEFULNESS OF FINANCIAL STATEMENT ANALYSIS FOR FORECASTING REAL ECONOMIC ACTIVITY

Descriptive Statistics

Table 1, Panel A provides descriptive statistics for aggregate accounting profitability ratios. All accounting variables are measured at the end of the first month, month t , after quarter q ends. At the aggregate level, accounting profitability ($RNOA$) has fluctuated between 15.8 percent and 39.5 percent, with a mean of 27.2 percent and a standard deviation of 4.5 percent. Aggregate changes in accounting profitability ($\Delta RNOA$) exhibit substantial time-series variation, with a mean of -0.4 percent and a standard deviation of 3.9 percent. Aggregate changes in $RNOA$ are decomposed into changes in asset turnover (ΔATO) and profit margin (ΔPM). The descriptive statistics show that the variability of $\Delta RNOA$ is driven by time-series variation in aggregate changes in both drivers. Untabulated statistics indicate that aggregate changes in $RNOA$ and its drivers are mostly autocorrelated at one lag. Based on the BEA's advance NIPA reports, the annualized growth rate in the real U.S. economy has fluctuated between -6.1 percent and 8.7 percent, with a mean of 2.5 percent and a standard deviation of 2.4 percent.

Table 1, Panel B reports pairwise correlations between aggregate changes in the profitability ratios and subsequent real GDP growth. The correlation matrix provides preliminary evidence that aggregate changes in $RNOA$ and its drivers contain leading information about real economic activity. Indeed, the correlation between aggregate changes in $RNOA$ and subsequent real GDP growth is 0.30. Across accounting profitability drivers, the strongest univariate correlation is 0.48 between aggregate changes in operating margins (ΔOM) and subsequent real GDP growth.

Predictive Content of Aggregate Changes in Accounting Profitability Drivers

Is financial statement analysis of firm profitability drivers applied at the aggregate level useful for macro forecasting? We address this question by estimating time-series regressions of subsequent real GDP growth on aggregate changes in $RNOA$ and the DuPont drivers, as follows:

$$g_{q+1} = \alpha + \sum_{\kappa} \beta_{\kappa} \times \Delta Profitability Ratio^{\kappa}_q + \varepsilon_{q+1}.$$

(2)

Throughout the study, we estimate our regression models using ordinary least squares regressions, and we base our statistical inferences on Newey and West (1987) standard errors and two-sided p-values. For the Newey and West (1987) standard errors, the lag length is based on the number of statistically significant residual autocorrelations. Across all our models, the autocorrelations of the estimated regression residuals are not significant beyond lag 4, so we set

TABLE 1
Descriptive Statistics

Panel A: Empirical Distributions

	Mean	Std. Dev.	Min.	Q1	Median	Q3	Max.
$RNOA_q$	0.272	0.045	0.158	0.247	0.271	0.302	0.395
ATO_q	1.817	0.279	1.322	1.640	1.791	1.952	2.625
PM_q	0.170	0.046	0.079	0.132	0.177	0.210	0.245
OM_q	0.233	0.048	0.138	0.198	0.244	0.274	0.323
DEP_q	0.063	0.007	0.047	0.059	0.062	0.066	0.090
$\Delta RNOA_q$	-0.004	0.039	-0.113	-0.030	0.000	0.019	0.095
ΔATO_q	-0.046	0.144	-0.482	-0.111	-0.020	0.051	0.248
ΔPM_q	0.002	0.016	-0.039	-0.008	0.001	0.011	0.055
ΔOM_q	0.003	0.013	-0.026	-0.005	0.000	0.010	0.050
ΔDEP_q	0.001	0.006	-0.015	-0.003	0.000	0.004	0.023
g_{q+1}	0.025	0.024	-0.061	0.014	0.025	0.039	0.087

Panel B: Pairwise Correlations

	$\Delta RNOA_q$	ΔATO_q	ΔPM_q	ΔOM_q	ΔDEP_q	g_{q+1}
$\Delta RNOA_q$	1.00	0.89	0.82	0.70	-0.68	0.30
		<i>< 0.001</i>	<i>< 0.001</i>	<i>< 0.001</i>	<i>< 0.001</i>	<i>< 0.001</i>
ΔATO_q		1.00	0.64	0.49	-0.67	0.18
			<i>< 0.001</i>	<i>< 0.001</i>	<i>< 0.001</i>	<i>0.05</i>
ΔPM_q			1.00	0.94	-0.65	0.37
				<i>< 0.001</i>	<i>< 0.001</i>	<i>< 0.001</i>
ΔOM_q				1.00	-0.36	0.48
					<i>< 0.001</i>	<i>< 0.001</i>
ΔDEP_q					1.00	0.03
						<i>0.73</i>
g_{q+1}						1.00

Panel A reports descriptive statistics for the quarterly time-series of the following aggregate profitability ratios: the rate of return on net operating assets (*RNOA*) measured as the ratio of annualized operating income after depreciation to average net operating assets; asset turnover (*ATO*) measured as the ratio of annualized sales to average net operating assets; profit margin (*PM*) measured as the ratio of operating income after depreciation to sales; operating margin (*OM*) measured as the ratio of operating income before depreciation to sales; and the depreciation-to-sales ratio (*DEP*). Operating income before depreciation is measured as sales minus cost of goods sold minus selling, general, and administrative expenses. Net operating assets are measured as operating assets (i.e., total assets less cash and short-term investments) minus operating liabilities (i.e., total liabilities less long-term debt less short-term debt). Aggregate year-over-year changes in profitability and profitability drivers are indicated with Δ . We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month *t*, after quarter *q* ends. g_{q+1} is the annualized real GDP growth obtained from the BEA advance NIPA reports. Panel B reports Pearson correlations and two-sided p-values (in italic). The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

the lag length equal to 4. None of our inferences are sensitive as to whether we use ordinary least squares standard errors, White (1980) standard errors, or Newey and West (1987) standard errors with lag length varying from one to four quarters.

Table 2 reports the results. In line with the pairwise correlations, column 1 of Table 2 documents that $\Delta RNOA$ is a significant leading indicator of real GDP growth. The estimated

TABLE 2
Predictive Content of Aggregate Changes in Accounting Profitability Drivers for Subsequent Real GDP Growth

	Dependent Variable = g_{q+1}				
	1	2	3	4	5
Intercept	0.026	0.027	0.024	0.023	0.023
t-statistic	9.11	8.30	8.47	7.34	8.21
p-value	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
$\Delta RNOA_q$	0.19				
t-statistic	2.69				
p-value	0.01				
ΔATO_q		0.03		-0.02	0.02
t-statistic		1.94		-0.81	1.39
p-value		0.05		0.42	0.17
ΔPM_q			0.55	0.64	
t-statistic			2.84	2.54	
p-value			0.01	0.01	
ΔOM_q					0.95
t-statistic					3.74
p-value					< 0.001
ΔDEP_q					1.26
t-statistic					3.34
p-value					< 0.001
Adjusted R^2	8%	2%	13%	13%	26%

This table reports results from time-series regressions of subsequent real GDP growth (g_{q+1}) on our quarterly indices of aggregate changes in profitability and profitability drivers. We obtain data on subsequent annualized real GDP growth from the BEA's quarterly advance NIPA reports. We measure quarterly changes in profitability as the year-over-year changes in the rate of return on net operating assets ($\Delta RNOA$). We decompose $\Delta RNOA$ into changes in profit margin (ΔPM) and changes in asset turnover (ΔATO). Changes in profit margin are decomposed into changes in operating margin (ΔOM) and changes in the depreciation-to-sales ratio (ΔDEP). We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month t , after quarter q ends. The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

coefficient on $\Delta RNOA$ is significantly positive at the 1 percent level with a t-statistic of 2.69. The magnitude of the estimated coefficient implies that a one-standard-deviation increase in $\Delta RNOA$ is associated with a 0.74 percentage point increase in subsequent real GDP growth. Aggregate changes in accounting profitability explain 8 percent of the time-series variation in subsequent real GDP growth.⁶

Decomposing $\Delta RNOA$ provides incremental information for forecasting real economic activity. Columns 2 and 3 of Table 2 document that, when considered separately, aggregate changes in asset turnover (ΔATO) and profit margins (ΔPM) are significant predictors of one-quarter-ahead real GDP growth. However, the predictive ability of ΔPM swamps that of ΔATO . Indeed, column 4 documents that when ΔPM and ΔATO are included together as predictors in Equation (2), the

⁶ In additional analysis, we do not find evidence of differential predictive ability of positive and negative aggregate changes in accounting profitability.

estimated coefficient on ΔATO is no longer significant, while that on ΔPM remains significantly positive.⁷ Looking across columns 1 through 4, it is clear that the predictive power of aggregate accounting profitability drivers for subsequent real GDP growth is primarily driven by ΔPM . On a stand-alone basis, aggregate changes in profit margins explain 13 percent of the time-series variation in subsequent real GDP growth.

As noted in Section III, changes in profit margins are traced to changes in operating margins (ΔOM) and changes in the depreciation-to-sales ratio (ΔDEP). Column 5 of Table 2 shows that the estimated coefficients on ΔOM and ΔDEP are significantly positive at the 1 percent level with t-statistics of 3.74 and 3.34, respectively.⁸ The magnitudes of the estimated coefficients imply that a one-standard-deviation increase in ΔOM is associated with a 1.24 percentage point increase in subsequent real GDP growth, while a one-standard-deviation increase in ΔDEP is associated with a 0.76 percentage point increase in subsequent real GDP growth. Overall, aggregate changes in accounting profitability drivers explain 26 percent of the time-series variation in one-quarter-ahead real GDP growth.⁹

The significantly positive association between ΔOM and subsequent GDP growth is consistent with prior evidence on the predictive content of margins for subsequent economic activity at the firm level (Ou and Penman 1989; Abarbanell and Bushee 1998; Kothari 2001). The significantly positive association between ΔDEP and subsequent GDP growth is consistent with Cheng's (2005) finding that the depreciation-to-sales ratio positively predicts economic activity at the firm level. This finding is also consistent with evidence in Ou and Penman (1989) and Barth, Cram, and Nelson (2001) that depreciation deflated by various proxies for scale positively predicts economic activity at the firm level.¹⁰

To summarize, consistent with our conjecture, we find that financial statement analysis of firm profitability drivers applied at the aggregate level yields timely insights about future real economic activity. At the minimum, our findings establish that aggregate changes in accounting profitability and its drivers are correlated with leading information about the prospects of the U.S. real economy.

Predictive Content of Stock Market Returns

Rational expectations asset pricing models suggest that stock market prices are related to investors' expectations about future economic activity (Fama 1981; Fischer and Merton 1984; Barro 1990; Fama 1990). To examine the predictive power of the stock market for real economic activity, we estimate time-series regressions of quarter $q+1$ real GDP growth on buy-and-hold stock market returns, $ret_{t-\tau \rightarrow t}$, measured over the $\tau = 3, 6, 12$, and 24 months leading to the end of the first

⁷ In additional analysis, we do not find evidence that the interaction of ΔATO with ΔPM has predictive content for subsequent GDP growth.

⁸ Comparing the multivariate regression results (Table 2) with the pairwise correlations (Table 1, Panel B) reveals that the predictive power of ΔDEP for subsequent GDP growth is muted when correlated changes in the other accounting profitability drivers are omitted.

⁹ In additional analysis, we examine the predictive content of the components of aggregate changes in asset turnover, including property, plant, and equipment turnover, inventory turnover, and receivables turnover. We do not find evidence that asset turnover components have incremental predictive content for subsequent real GDP growth. When we decompose aggregate changes in operating margins, we also do not find evidence of incremental predictive content for operating margin components, including changes in sales-deflated ratios of cost of goods sold, selling, general, and administrative expenses, as well as research and development expenses.

¹⁰ The predictive ability of ΔDEP for subsequent real GDP growth is not driven by the deflator of the depreciation-to-sales ratio. First, we find similar results when we include aggregate sales growth as an additional regressor in the right-hand side of Equation (2). Second, we find similar results using different deflators, including current and lagged amounts of book value of total assets, book value of equity, market value of equity, and sales. Also, we note that the estimated persistence of DEP is 0.63, which is significantly different from 1 using two-tailed F-tests.

month, month t , after quarter q ends:

$$g_{q+1} = \alpha + \beta_{\tau} \times ret_{t-\tau \rightarrow t} + \varepsilon_{q+1}. \quad (3)$$

The return measurement window is aligned with the timing of the SPF, thus ensuring that the different measures of stock market returns that we employ are available to macro forecasters. Figure 1, Panel B illustrates the measurement of $ret_{t-\tau \rightarrow t}$. Table 3, Panels A and B present results based on Equation (3) using the CRSP index and the S&P 500 index, respectively.

Consistent with rational expectations asset pricing models, we find that stock market returns contain significant leading information about real economic activity. Table 3 shows that stock market returns positively predict quarter $q+1$ real GDP growth, with their predictive power varying with the length of the measurement window. The predictive power of stock market returns initially rises as we stretch the measurement window over the 12 months preceding month t , but it then declines beyond one year. The evidence shows that the 12-month stock market return is the most powerful for macro forecasting, explaining 20 percent of the time-series variation in quarter $q+1$ real GDP growth. This inference holds for both the CRSP index and the S&P 500 index.

Our findings establish that stock market investors anticipate a significant fraction of the variation in real economic activity one year in advance. Given the significant predictive power of stock market returns, the next question becomes whether aggregate accounting profitability drivers are *incrementally useful* for macro forecasting.

Incremental Predictive Content of Aggregate Changes in Accounting Profitability Drivers

From a practical implementation standpoint, the predictive content of aggregate accounting profitability drivers is relevant for macro forecasting only to the extent that it is incremental to the predictive content of stock market returns. This is because collecting and aggregating income statement and balance sheet data entail non-trivial costs, while stock market return data are readily available to macro forecasters.

Table 4 reports results from time-series regressions of subsequent real GDP growth on aggregate changes in accounting profitability drivers and stock market returns, as follows:

$$g_{q+1} = \alpha + \beta_1 \times \Delta ATO_q + \beta_2 \times \Delta OM_q + \beta_3 \times \Delta DEP_q + \beta_4 \times ret_{t-12 \rightarrow t} + \varepsilon_{q+1}. \quad (4)$$

Equation (4) provides direct evidence on the marginal predictive content of accounting profitability drivers after controlling for annual stock market returns, $ret_{t-12 \rightarrow t}$. We focus on annual stock market returns because our findings in Table 3 indicate that the predictive power of the stock market for quarter $q+1$ real GDP growth peaks when measuring returns over the 12 months preceding month t . Columns 1 and 2 of Table 4 report results using annual stock market returns based on the CRSP index and the S&P 500 index, respectively.

The key message from Table 4 is that financial statement analysis of firm profitability drivers applied at the aggregate level is relevant for forecasting real economic activity. More specifically, aggregate changes in operating margins and the depreciation-to-sales ratio contain leading information for subsequent real GDP growth *after* controlling for the predictive content of annual stock market returns. The estimated coefficients on ΔOM and ΔDEP remain significantly positive at the 1 percent level with t-statistics in excess of 3. The magnitudes of the estimated coefficients imply that a one-standard-deviation increase in ΔOM is associated with a 1.03 percentage point increase in subsequent real GDP growth, while a one-standard-deviation increase in ΔDEP is associated with a 0.59–0.61 percentage point increase in subsequent real GDP growth.

Clearly, the use of aggregate accounting profitability data leads to significant improvements in terms of explanatory power with the adjusted R^2 rising from 20 percent, when annual stock market returns are included as stand-alone predictors of subsequent real GDP growth, to 36 percent, when

TABLE 3
Predictive Content of Stock Market Returns for Subsequent Real GDP Growth

Panel A: Proxy for the Stock Market Portfolio is the CRSP Index

	Dependent Variable = g_{q+1}			
	1	2	3	4
Intercept	0.023	0.021	0.018	0.018
t-statistic	6.22	5.24	4.69	3.77
p-value	< 0.001	< 0.001	< 0.001	< 0.001
$ret_{t-3 \rightarrow t}^{CRSP}$	0.09			
t-statistic	2.12			
p-value	0.04			
$ret_{t-6 \rightarrow t}^{CRSP}$		0.08		
t-statistic		2.96		
p-value		< 0.001		
$ret_{t-12 \rightarrow t}^{CRSP}$			0.06	
t-statistic			3.88	
p-value			< 0.001	
$ret_{t-24 \rightarrow t}^{CRSP}$				0.03
t-statistic				2.20
p-value				0.03
Adjusted R ²	7%	15%	20%	7%

Panel B: Proxy for the Stock Market Portfolio is the S&P 500 Index

	Dependent Variable = g_{q+1}			
	1	2	3	4
Intercept	0.023	0.021	0.019	0.020
t-statistic	6.57	5.85	5.45	4.51
p-value	< 0.001	< 0.001	< 0.001	< 0.001
$ret_{t-3 \rightarrow t}^{S\&P}$	0.09			
t-statistic	2.14			
p-value	0.03			
$ret_{t-6 \rightarrow t}^{S\&P}$		0.09		
t-statistic		3.04		
p-value		< 0.001		
$ret_{t-12 \rightarrow t}^{S\&P}$			0.06	
t-statistic			3.85	
p-value			< 0.001	
$ret_{t-24 \rightarrow t}^{S\&P}$				0.03
t-statistic				2.46
p-value				0.02
Adjusted R ²	8%	16%	20%	8%

This table reports results from time-series regressions of subsequent real GDP growth (g_{q+1}) on buy-and-hold stock market returns (ret). Stock market returns are measured over the $\tau = 3, 6, 12$, and 24 months leading to the end of the first month, month t , after quarter q ends. We obtain data on annualized real GDP growth from the BEA’s quarterly advance NIPA reports. Panel A reports results using the CRSP index (CRSP) as our proxy for the stock market portfolio. Panel B reports results using the S&P 500 index (S&P) as our proxy for the stock market portfolio. The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

TABLE 4

Incremental Predictive Content of Aggregate Changes in Accounting Profitability Drivers and Stock Market Returns for Subsequent Real GDP Growth

	Dependent Variable = g_{q+1}	
	1	2
Intercept	0.018	0.019
t-statistic	5.83	6.45
p-value	< 0.001	< 0.001
ΔATO_q	0.01	0.01
t-statistic	0.73	0.65
p-value	0.47	0.52
ΔOM_q	0.79	0.79
t-statistic	3.58	3.62
p-value	< 0.001	< 0.001
ΔDEP_q	1.01	0.98
t-statistic	3.12	3.03
p-value	< 0.001	< 0.001
$ret_{t-12 \rightarrow t}^{CRSP}$	0.04	
t-statistic	3.63	
p-value	< 0.001	
$ret_{t-12 \rightarrow t}^{S\&P}$		0.05
t-statistic		3.66
p-value		< 0.001
Adjusted R ²	36%	36%

This table reports results from time-series regressions of subsequent real GDP growth (g_{q+1}) on our quarterly indices of aggregate changes in profitability drivers and stock market returns. We obtain data on annualized real GDP growth from the BEA’s quarterly advance NIPA reports. We measure quarterly changes in profitability drivers as year-over-year changes in asset turnover (ΔATO), operating margin (ΔOM), and the depreciation-to-sales ratio (ΔDEP). We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month t , after quarter q ends. Stock market returns (ret) are measured over the 12 months leading to the end of the first month, month t , after quarter q ends. We proxy for the stock market portfolio using the CRSP index (CRSP) and the S&P 500 index (S&P). The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

annual stock market returns are included together with aggregate changes in accounting profitability drivers. Overall, the predictive content of aggregate accounting profitability data is incremental to that of stock market returns, suggesting that financial statement analysis applied at the aggregate level is *incrementally useful* for macro forecasting.

V. THE USE OF FINANCIAL STATEMENT ANALYSIS BY MACRO FORECASTERS

The analysis so far shows that aggregate changes in profitability drivers and stock market returns are *incrementally useful* for forecasting real economic activity. We now attempt to shed light on the *use* of financial statement analysis by professional macro forecasters.

Explaining Macro Forecasters’ Revisions in Expectations

To begin, we test whether professional macro forecasters respond to aggregate changes in profitability drivers and stock market returns when revising their projections of quarter $q+1$ real

TABLE 5
Association of Revisions in Expectations about Subsequent Real GDP Growth with Aggregate Changes in Accounting Profitability Drivers and Stock Market Returns

	Dependent Variable = $E_q[g_{q+1}] - E_{q-1}[g_{q+1}]$	
	1	2
Intercept	-0.007	-0.006
t-statistic	-4.59	-4.30
p-value	< 0.001	< 0.001
ΔATO_q	0.00	0.00
t-statistic	0.24	0.20
p-value	0.81	0.84
ΔOM_q	0.29	0.29
t-statistic	3.51	3.65
p-value	< 0.001	< 0.001
ΔDEP_q	0.35	0.34
t-statistic	2.03	1.95
p-value	0.04	0.05
$ret_{t-12 \rightarrow t}^{CRSP}$	0.02	
t-statistic	3.57	
p-value	< 0.001	
$ret_{t-12 \rightarrow t}^{S\&P}$		0.02
t-statistic		3.37
p-value		< 0.001
Adjusted R ²	22%	22%

This table reports results from time-series regressions of the quarter $q-1$ to quarter q revision of the mean consensus forecast of quarter $q+1$ real GDP growth from the SPF, denoted $E_q[g_{q+1}] - E_{q-1}[g_{q+1}]$, on our quarterly indices of aggregate changes in profitability drivers and stock market returns. We obtain SPF mean consensus forecasts from the Federal Reserve Bank of Philadelphia. We measure quarterly changes in profitability drivers as year-over-year changes in asset turnover (ΔATO), operating margin (ΔOM), and the depreciation-to-sales ratio (ΔDEP). We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month t , after quarter q ends. Stock market returns (ret) are measured over the 12 months leading to the end of the first month, month t , after quarter q ends. We proxy for the stock market portfolio using the CRSP index (CRSP) and the S&P 500 index (S&P). The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

GDP growth. Table 5 reports results from time-series regressions of SPF consensus forecast revisions from quarter $q-1$ to quarter q of quarter $q+1$ real GDP growth, denoted $E_q[g_{q+1}] - E_{q-1}[g_{q+1}]$, on aggregate changes in accounting profitability drivers and annual stock market returns, as follows:

$$E_q[g_{q+1}] - E_{q-1}[g_{q+1}] = \alpha + \beta_1 \times \Delta ATO_q + \beta_2 \times \Delta OM_q + \beta_3 \times \Delta DEP_q + \beta_4 \times ret_{t-12 \rightarrow t} + \varepsilon_q. \tag{5}$$

If macro forecasters revise their real GDP growth expectations in the direction of the predictive content of aggregate changes in *RNOA* drivers and stock market returns, then based on Equation (5) we conjecture that $\beta_1 = 0$, $\beta_2 > 0$, $\beta_3 > 0$, and $\beta_4 > 0$. This conjecture follows from the evidence in Table 4 that ΔATO has no incremental predictive content, while ΔOM , ΔDEP , and stock market returns are significantly positive predictors of real GDP growth.

The findings, presented in Table 5, are consistent with our conjectures. While unrelated to aggregate changes in asset turnover, macro forecasters' revisions in expectations are positively associated with aggregate changes in operating margins and the depreciation-to-sales ratio as well as with stock market returns. Taken together, aggregate changes in accounting profitability drivers and stock market returns explain 22 percent of the time-series variation in macro forecasters' revisions in expectations of quarter $q+1$ real GDP growth.

Interestingly, the estimated intercepts based on Equation (5) are significantly negative, suggesting that professional macro forecasters tend to revise their real GDP growth forecasts downward as the BEA's scheduled NIPA release dates approach. This result provides the first evidence of a "walk-down" in macro forecasts and extends results in firm-level accounting research that long-term optimism in analysts' earnings forecasts decreases as earnings announcement dates approach (Brown, Foster, and Noreen 1985; Richardson, Teoh, and Wysocki 2004).

Our inferences hold for both the CRSP index and the S&P 500 index and are consistent with the findings presented in Tables 2, 3, and 4, indicating that macro forecasters revise their expectations of quarter $q+1$ real GDP growth in the direction of the predictive content of aggregate accounting profitability drivers and stock market returns. In particular, macro forecasters interpret aggregate increases in operating margins and the depreciation-to-sales ratio as well as stock market returns as positive for future real GDP growth. These findings, however, do not necessarily imply that macro forecasters fully impound the predictive content of aggregate accounting profitability drivers and stock market returns when forecasting real economic activity.

Are Macro Forecasters' Errors Predictable?

Do professional macro forecasters fully impound the predictive content of aggregate accounting profitability drivers and stock market returns? To address this question, we estimate time-series regressions of subsequent real GDP growth forecast errors on aggregate changes in accounting profitability drivers and stock market returns, as follows:

$$g_{q+1} - E_q[g_{q+1}] = \alpha + \beta_1 \times \Delta ATO_q + \beta_2 \times \Delta OM_q + \beta_3 \times \Delta DEP_q + \beta_4 \times ret_{t-12 \rightarrow t} + \varepsilon_{q+1}. \quad (6)$$

If professional macro forecasters fully impound the predictive content of aggregate changes in *RNOA* drivers and stock market returns when revising their expectations about quarter $q+1$ real GDP growth, then their subsequent forecast errors, denoted $g_{q+1} - E_q[g_{q+1}]$, should not be predictable and the estimated slope coefficients based on Equation (6) should not be different from 0. As discussed in Section II, it is unclear whether professional macro forecasters are fully attuned to the idea that financial statement analysis of firm profitability drivers applied at the aggregate level can be useful for macro forecasting. In contrast, we have reasons to believe that they are attuned to the idea that stock market returns are useful for macro forecasting.

If macro forecasters are fully attuned to stock market return data but not to aggregate accounting profitability data, then based on Equation (6) we conjecture that $\beta_1 = 0$, $\beta_2 > 0$, $\beta_3 > 0$, and $\beta_4 = 0$. This conjecture also follows from the findings in Table 4 that show ΔATO has no incremental predictive content, while ΔOM and ΔDEP are significantly positive predictors of real GDP growth.

The findings, presented in Table 6, are consistent with the above conjecture. Specifically, column 1 of Table 6 documents that, while unrelated to aggregate changes in asset turnover, real GDP growth forecast errors are predictable based on aggregate changes in operating margins and the depreciation-to-sales ratio. The estimated coefficients on ΔOM and ΔDEP are significantly different from zero. The positive signs of the estimated coefficients on ΔOM and ΔDEP are directionally consistent with the predictive content of these variables and imply that professional

TABLE 6
Predictive Content of Aggregate Changes in Accounting Profitability Drivers and Stock Market Returns for Subsequent Real GDP Growth Forecast Errors

	Dependent Variable = $g_{q+1} - E_q[g_{q+1}]$				
	1	2	3	4	5
Intercept	0.001	0.002	0.001	0.001	0.001
t-statistic	1.02	0.99	1.02	0.86	0.75
p-value	0.31	0.32	0.31	0.39	0.45
ΔATO_q	0.00			0.00	0.00
t-statistic	0.31			0.29	0.23
p-value	0.75			0.78	0.82
ΔOM_q	0.30			0.30	0.29
t-statistic	3.17			3.05	2.94
p-value	< 0.001			< 0.001	< 0.001
ΔDEP_q	0.78			0.77	0.76
t-statistic	2.34			2.25	2.15
p-value	0.02			0.03	0.03
$ret_{t-12 \rightarrow t}^{CRSP}$		0.01		0.00	
t-statistic		1.07		0.24	
p-value		0.29		0.81	
$ret_{t-12 \rightarrow t}^{S\&P}$			0.01		0.00
t-statistic			1.44		0.60
p-value			0.15		0.55
Adjusted R ²	8%	0%	1%	7%	7%

This table reports results from time-series regressions of subsequent real GDP growth forecast errors on our quarterly indices of aggregate changes in profitability drivers and stock market returns. We measure subsequent real GDP growth forecast errors as the difference between subsequent real GDP growth and the corresponding mean consensus forecast as of quarter q from the SPF, denoted $g_{q+1} - E_q[g_{q+1}]$. We obtain data on subsequent annualized real GDP growth from the BEA's quarterly advance NIPA reports. We obtain the SPF mean consensus forecasts of real GDP growth from the Federal Reserve Bank of Philadelphia. We measure quarterly changes in profitability drivers as year-over-year changes in asset turnover (ΔATO), operating margin (ΔOM), and the depreciation-to-sales ratio (ΔDEP). We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month t , after quarter q ends. Stock market returns (ret) are measured over the 12 months leading to the end of the first month, month t , after quarter q ends. We proxy for the stock market portfolio using the CRSP index (CRSP) and the S&P 500 index (S&P). The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

macro forecasters do not fully impound the informativeness of aggregate changes in operating margins and the depreciation-to-sales ratio for subsequent real GDP growth.

The magnitudes of the estimated slope coefficients imply that real GDP growth forecasts can be improved in an economically significant way using aggregate accounting profitability drivers: a one-standard-deviation increase in ΔOM is associated with a 0.39 percentage point increase in subsequent real GDP growth forecast error, while a one-standard-deviation increase in ΔDEP is associated with a 0.47 percentage point increase in subsequent real GDP growth forecast error. Taken together, aggregate changes in accounting profitability drivers explain 8 percent of the time-series variation in one-quarter-ahead real GDP growth forecast errors.

In contrast, the results in columns 2 and 3 of Table 6 show that stock market returns lack predictive power for subsequent real GDP growth forecast errors. The lack of association between

stock market returns and subsequent forecast errors implies that macro forecasters fully impound the predictive content of stock market returns when projecting real GDP growth. In line with this finding, columns 4 and 5 show that the predictive power of aggregate changes in operating margins and the depreciation-to-sales ratio for one-quarter-ahead real GDP growth forecast errors remain intact after controlling for stock market returns.

Viewed as a whole, our findings suggest that although professional macro forecasters are fully attuned to the predictive content of stock market returns, they do not fully impound the predictive content of aggregate accounting profitability drivers. Our evidence is consistent with the fact that stock market return data are known to have predictive ability for real economic activity (Fama 1981) and are readily available to macro forecasters, while aggregate accounting profitability data are not. Given that stock market returns do not subsume the predictive content of aggregate accounting profitability drivers for real GDP growth (Table 4) and real GDP growth forecast errors (Table 6), it follows that investors’ projections of the real economy embedded in stock market prices can also be improved based on aggregate accounting profitability data.

Implications for Stock Valuation

We now examine the association between stock market returns and the portion of quarter $q+1$ real GDP growth that is predictable based on our indices of aggregate accounting profitability data but that is not anticipated by stock market investors. Specifically, we estimate the following time-series regression:

$$ret_{t+1 \rightarrow t+3} = \alpha + \beta \times g_{q+1}^{ACC} + \varepsilon_{t+1 \rightarrow t+3}.$$
 (7)

The left-hand-side variable in Equation (7) is the buy-and-hold stock market return measured over the three-month period from the end of month t to the end of month $t+3$. The return measurement window allows us to capture information flows leading to the BEA’s advance release of real GDP growth for quarter $q+1$, which occurs by the end of the first month after quarter $q+1$ ends, i.e., end of month $t+3$. Figure 1, Panel B illustrates the measurement of $ret_{t+1 \rightarrow t+3}$.

The right-hand-side variable in Equation (7) is measured in two stages. In the first stage, we obtain the fitted values from the following time-series regression of subsequent real GDP growth on aggregate changes in accounting profitability drivers:

$$g_{q+1} = \alpha + \beta_1 \times \Delta ATO_q + \beta_2 \times \Delta OM_q + \beta_3 \times \Delta DEP_q + \varepsilon_{q+1}.$$
 (8)

In the second stage, we regress the fitted values from Equation (8) on annual stock market returns measured over the 12 months leading to the end of month t and obtain the residuals. The residuals from this second-stage regression, denoted g_{q+1}^{ACC} , capture the portion of subsequent real GDP growth that is predictable based on our indices of aggregate accounting profitability drivers but that is not anticipated by stock market investors.

Table 7 reports results based on Equation (8).¹¹ Columns 1 and 3 document a significantly positive association between stock market returns and the predictable portion of real GDP growth for the CRSP index and the S&P index, respectively. Columns 2 and 4 expand the right-hand side of Equation (8) to include the portions of aggregate changes in *RNOA* drivers that are unrelated to g_{q+1}^{ACC} , denoted ΔATO^{res} , ΔOM^{res} , and ΔDEP^{res} . The estimated coefficients on g_{q+1}^{ACC} remain intact, while the estimated coefficients on ΔATO^{res} , ΔOM^{res} , and ΔDEP^{res} are insignificantly different from zero. This finding suggests that aggregate accounting profitability drivers flow into stock

¹¹ To ease interpretation of the estimated coefficients, we standardize the right-hand-side variables in Table 7 to have a mean of 0 and a standard deviation of 1. Our inferences are identical using the raw values of the regressors.

TABLE 7
Association between Stock Market Returns and Predictable Real GDP Growth
Dependent Variable =

	$ret_{t+1 \rightarrow t+3}^{CRSP}$		$ret_{t+1 \rightarrow t+3}^{S\&P}$	
	1	2	3	4
Intercept	0.028	0.028	0.021	0.021
t-statistic	3.64	3.42	2.80	2.67
p-value	< 0.001	< 0.001	0.01	0.01
g_{q+1}^{ACC}	0.02	0.02	0.01	0.01
t-statistic	2.18	2.18	2.24	2.28
p-value	0.03	0.03	0.03	0.02
ΔATO_q^{res}		-0.002		-0.003
t-statistic		-0.12		-0.22
p-value		0.91		0.82
ΔOM_q^{res}		-0.014		-0.008
t-statistic		-1.06		-0.61
p-value		0.29		0.54
ΔDEP_q^{res}		-0.002		0.002
t-statistic		-0.21		0.22
p-value		0.83		0.83
Adjusted R ²	3%	4%	3%	3%

This table reports results from time-series regressions of the three-month buy-and-hold stock market return (ret) measured after the end of the first month, month t , after quarter q ends on the following variables: (i) g_{q+1}^{ACC} , the predictable portion of subsequent real GDP growth measured in two stages. In the first stage, we obtain the fitted values from a time-series regression of subsequent real GDP growth on aggregate changes in asset turnover (ΔATO), aggregate changes in operating margins (ΔOM), and aggregate changes in the depreciation-to-sales ratio (ΔDEP). We construct the aggregate time-series using value-weighted cross-sectional averages based on the 100 largest U.S. listed firms in terms of lagged market capitalization with accounting data released by the end of the first month, month t , after quarter q ends. In the second stage, we orthogonalize these fitted values on stock market returns measured over the 12 months leading to the end of month t ; (ii) ΔATO_q^{res} , the residual from a regression of ΔATO on the predictable portion of subsequent GDP growth; (iii) ΔOM_q^{res} , the residual from a regression of ΔOM on the predictable portion of subsequent GDP growth; and (iv) ΔDEP_q^{res} , the residual from a regression of ΔDEP on the predictable portion of subsequent GDP growth. Variables (i) through (iv) are standardized to have a mean of 0 and a standard deviation of 1. We proxy for the stock market portfolio using the CRSP index (CRSP) and the S&P 500 index (S&P). The sample period includes 120 quarters from 1981:Q3 to 2011:Q3.

market returns through subsequent real GDP growth. In additional month-by-month analysis, we find that the link between the predictable portion of subsequent real GDP growth and stock market returns flows evenly over the three months leading to the BEA’s advance release of real GDP growth for quarter $q+1$.

There are at least two potential explanations for our finding of a positive association between stock market returns and the portion of real GDP growth that is predictable based on aggregate accounting profitability data. First, it may be due to delayed assimilation of aggregate accounting profitability drivers. Second, it may be due to a positive link between investors’ expectations about discount rates and investors’ expectations about growth. Indeed, prior research suggests that there is a common component between revisions in expectations about discount rates and revisions in

expectations about growth.¹² We remain agnostic about the appropriate explanation. Nevertheless, the evidence we document suggests that the link between aggregate accounting profitability drivers and subsequent real GDP growth is relevant for stock valuation.

VI. CONCLUSION

Our study investigates the usefulness of financial statement analysis of firm profitability drivers for forecasting real economic activity. We first document that focusing on the 100 largest firms offers a cost-effective way to extract information in accounting profitability data of the entire stock market portfolio. We then show that accounting profitability data aggregated across the 100 largest firms have predictive ability for subsequent real GDP growth. We also show that stock market returns predict subsequent real GDP growth, while their predictive power varies with the length of the measurement window with annual stock market returns being the most powerful. Importantly, we find that the predictive content of our indices of aggregate accounting profitability drivers is not subsumed by that of stock market returns, suggesting that financial statement analysis of firm profitability at the aggregate level is *incrementally useful* for macro forecasting.

An in-depth investigation of consensus forecasts from the SPF shows that professional macro forecasters revise their expectations of real economic activity in the direction of the predictive content of aggregate accounting profitability drivers and stock market returns. Although macro forecasters are fully attuned to stock market return data, their forecasts of real GDP growth can be improved in a statistically and economically significant way using our indices of aggregate accounting profitability drivers. Overall, our findings suggest that professional macro forecasters and stock market investors do not fully impound the predictive content of aggregate accounting profitability drivers for real economic activity. In additional analysis, we examine the association between stock market returns and the portion of subsequent real GDP growth that is predictable based on our indices of aggregate accounting profitability drivers but that is not anticipated by stock market investors. We find that this portion is positively related to stock market returns, suggesting that the predictive content of aggregate accounting profitability drivers for real economic activity is relevant for stock valuation.

Notwithstanding our evidence that financial statement analysis of firm profitability drivers applied at the aggregate level is an incrementally useful tool for taking the pulse of the U.S. real economy, we acknowledge that collecting and aggregating income statement and balance sheet data impose non-trivial information acquisition and processing costs on macro forecasters. Our indices of aggregate firm profitability drivers, however, are based on the 100 largest firms with accounting data released within one month after the quarter ends. Our study highlights that focusing on this set of firms can offer insights into the performance of the entire stock market portfolio, limiting the practical costs associated with collecting and aggregating data from corporate financial reports. We also acknowledge that aggregate indices mask heterogeneity across firms. Future research may find cross-sectional variation in the usefulness of financial statement analysis for macro forecasting.

¹² Campbell and Shiller (1988) and Campbell (1991) decompose stock returns into expected returns, cash flow news, and discount rate news. With this decomposition in mind, prior research shows that there is a common component between cash flow news and discount rate news. Lettau and Ludvigson (2005) provide evidence of common variation in discount rates and expected growth rates. Cochrane (2011) notes that discount rates and expected growth rates are likely to covary positively over the business cycle. Konchitchki, Lou, G. Sadka, and R. Sadka (2014), motivated by investment-based asset pricing models, argue that high expected earnings can be obtained by undertaking risky projects, suggesting a positive relation at the firm level between expected returns and expected growth. Patatoukas (2013) finds that cash flow news and discount rate news in aggregate changes in accounting profitability covary positively over time and have offsetting impacts on stock market prices.

Such research would further broaden our knowledge of the link between accounting profitability drivers and the aggregate economy.¹³

REFERENCES

- Abarbanell, J. S., and B. J. Bushee. 1998. Abnormal returns to a fundamental analysis strategy. *The Accounting Review* 73 (1): 19–45.
- Baghestani, H., and A. M. Kianian. 1993. On the rationality of US macroeconomic forecasts: Evidence from a panel of professional forecasters. *Applied Economics* 25 (7): 869–878.
- Barro, R. J. 1990. The stock market and investment. *Review of Financial Studies* 3 (1): 115–131.
- Barth, M. E., D. P. Cram, and K. K. Nelson. 2001. Accruals and the prediction of future cash flows. *The Accounting Review* 76 (1): 27–58.
- Basu, S., S. Markov, and L. Shivakumar. 2010. Inflation, earnings forecasts, and post-earnings announcement drift. *Review of Accounting Studies* 15 (2): 403–440.
- Brown, L., G. Foster, and E. Noreen. 1985. *Security Analyst Multi-Year Earnings Forecasts and the Capital Market*. Studies in Accounting Research No. 23. Sarasota, FL: American Accounting Association.
- Bureau of Economic Analysis (BEA). 2007. *Measuring the Economy: A Primer on GDP and the National Income and Product Accounts*. United States Department of Commerce: Economics and Statistics Administration, Bureau of Economic Analysis.
- Campbell, J. Y. 1991. A variance decomposition for stock returns. *Economic Journal* 101 (405): 157–179.
- Campbell, J. Y., and R. J. Shiller. 1988. The dividend-price ratio and expectations of future dividends and discount factors. *Review of Financial Studies* 1 (3): 195–228.
- Cheng, Q. 2005. What determines residual income? *The Accounting Review* 80 (1): 85–112.
- Cochrane, J. H. 2011. Presidential address: Discount rates. *Journal of Finance* 66 (4): 1047–1108.
- Cready, W. M., and U. G. Gurun. 2010. Aggregate market reaction to earnings announcements. *Journal of Accounting Research* 48 (2): 289–334.
- Croushore, D. 2002. Comments on: The state of macroeconomic forecasting. *Journal of Macroeconomics* 24 (4): 483–489.
- Croushore, D. 2011. Real-time forecasting. In *Advances in Economic Forecasting*, 7–24. Kalamazoo, MI: Upjohn Institute.
- Fairfield, P. M., and T. L. Yohn. 2001. Using asset turnover and profit margin to forecast changes in profitability. *Review of Accounting Studies* 6 (4): 371–385.
- Fama, E. F. 1981. Stock returns, real activity, inflation, and money. *American Economic Review* 71 (4): 545–565.
- Fama, E. F. 1990. Stock returns, expected returns, and real activity. *The Journal of Finance* 45 (4): 1089–1108.
- Fildes, R., and H. Stekler. 2002. The state of macroeconomic forecasting. *Journal of Macroeconomics* 24 (4): 435–468.
- Fischer, S., and R. C. Merton. 1984. *Macroeconomics and Finance: The Role of the Stock Market*. Working Paper No. 1291. Cambridge, MA: National Bureau of Economic Research.
- Fixler, D. J., and B. T. Grimm. 2005. Reliability of the NIPA estimates of U.S. economic activity. *Survey of Current Business* 85 (2): 8–19.
- Graham, J. R. 1996. Is a group of economists better than one? Than none? *Journal of Business* 69 (2): 193–232.
- Konchitchki, Y. 2011. Inflation and nominal financial reporting: Implications for performance and stock prices. *The Accounting Review* 86 (3): 1045–1085.

¹³ In a follow-up study, we find evidence that macro forecasters can further enhance their projections of future real economic activity by paying closer attention to accounting data from bellwether firms and cyclical industries.

- Konchitchki, Y., X. Lou, G. Sadka, and R. Sadka. 2014. *Expected Earnings and the Post-Earnings-Announcement Drift*. Working paper.
- Konchitchki, Y., and P. N. Patatoukas. 2013. Accounting earnings and gross domestic product. *Journal of Accounting and Economics* (forthcoming). DOI: 10.1016/j.jacceco.2013.10.001. Available at: <http://www.sciencedirect.com/science/article/pii/S016541011300058X>
- Kothari, S. P. 2001. Capital markets research in accounting. *Journal of Accounting and Economics* 31 (1-3): 105–231.
- Kothari, S. P., J. Lewellen, and J. B. Warner. 2006. Stock returns, aggregate earnings surprises, and behavioral finance. *Journal of Financial Economics* 79 (3): 537–568.
- Kothari, S. P., L. Shivakumar, and O. Urcan. 2013. *Aggregate Earnings Surprises and Inflation Forecasts*. Working paper.
- Krane, S. 2003. An evaluation of real GDP forecasts: 1996–2001. *Federal Reserve Bank of Chicago Economic Perspectives* (Q1): 2–21.
- Lahiri, K., and J. G. Wang. 2006. Subjective probability forecasts for recessions: Evaluations and guidelines for use. *Business Economics* 41 (2): 26–37.
- Landefeld, J. S., E. P. Seskin, and B. M. Fraumeni. 2008. Taking the pulse of the economy: Measuring GDP. *Journal of Economic Perspectives* 22 (2): 193–216.
- Lettau, M., and S. C. Ludvigson. 2005. Expected returns and expected dividend growth. *Journal of Financial Economics* 76 (3): 583–626.
- Lev, B. 1983. Some economic determinants of time-series properties of earnings. *Journal of Accounting and Economics* 5 (1): 31–48.
- Lev, B., and R. Thiagarajan. 1993. Fundamental information analysis. *Journal of Accounting Research* 31 (2): 190–215.
- McNees, S. K. 1992. How large are economic forecast errors? *New England Economic Review* (July/August): 25–42.
- Merchant, K. A. 2012. Making management accounting more useful. *Pacific Accounting Review* 24 (3): 334–356.
- Moehrle, S. R., K. Anderson, F. L. Ayres, C. E. Bolt-Lett, R. S. Debreceeny, M. T. Dugan, C. E. Hogan, M. W. Maher, and E. Plummer. 2009. The impact of academic accounting research on professional practice: An analysis by the AAA Research Impact Task Force. *Accounting Horizons* 23 (4): 411–456.
- Newey, W. K., and K. D. West. 1987. A simple, positive semi-definite, heteroscedasticity and autocorrelation consistent covariance matrix. *Econometrica* 55 (3): 703–708.
- Nissim, D., and S. H. Penman. 2001. Ratio analysis and equity valuation: From research to practice. *Review of Accounting Studies* 6 (1): 109–154.
- Ou, J. A., and S. H. Penman. 1989. Financial statement analysis and the prediction of stock returns. *Journal of Accounting and Economics* 11 (4): 295–329.
- Patatoukas, P. N. 2012. Customer-base concentration: Implications for firm performance and capital markets. *The Accounting Review* 87 (2): 363–392.
- Patatoukas, P. N. 2013. Detecting news in aggregate accounting earnings: Implications for stock market valuation. *The Review of Accounting Studies* (forthcoming). DOI: 10.1007/S11142-013-9221-3. Available at: <http://link.springer.com/article/10.1007%2Fs11142-013-9221-3>
- Penman, S. H. 1992. Return to fundamentals. *Journal of Accounting, Auditing and Finance* 7 (Fall): 465–483.
- Penman, S. H. 2001. *Financial Statement Analysis and Security Valuation*. 1st edition. New York, NY: McGraw-Hill.
- Richardson, S., S. H. Teoh, and P. D. Wysocki. 2004. The walk-down to beatable analyst forecasts: The role of equity issuance and insider trading incentives. *Contemporary Accounting Research* 21 (4): 885–924.
- Schuh, S. 2001. An evaluation of recent macroeconomic forecast errors. *New England Economic Review* (January/February): 35–56.

- Shivakumar, L. 2010. Discussion of aggregate market reaction to earnings announcements. *Journal of Accounting Research* 48 (2): 335–342.
- Sims, C. A. 2002. The role of models and probabilities in the monetary policy process. *Brookings Papers on Economic Activity, Economic Studies Program, the Brookings Institution* 33 (2): 1–62.
- Soliman, M. T. 2008. The use of DuPont analysis by market participants. *The Accounting Review* 83 (3): 823–853.
- Stark, T. 2010. Realistic evaluation of real-time forecasts in the survey of professional forecasters. Federal Reserve Bank of Philadelphia, Special Report. Available at: <http://www.phil.frb.org/research-and-data/publications/research-rap/2010/realistic-evaluation-of-real-time-forecasts.pdf>
- Stock, J. H., and M. W. Watson. 2003. How did the leading indicator forecasts perform during the 2001 recession? *Federal Reserve Bank of Richmond, Economic Quarterly* 89 (3): 71–90.
- White, H. 1980. A heteroscedasticity-consistent covariance matrix estimator and a direct test for heteroscedasticity. *Econometrica* 48 (4): 817–838.
- Wieland, V., and M. H. Wolters. 2011. The diversity of forecasts from macroeconomic models of the U.S. economy. *Economic Theory* 47 (2): 247–292.
- Zarnowitz, V., and P. Braun. 1993. Twenty-two years of the NBER-ASA quarterly economic outlook surveys: Aspects and comparisons of forecasting performance. In *Business Cycles, Indicators, and Forecasting*, edited by J. Stock, and M. Watson. Chicago, IL: University of Chicago Press.

Firm-Value Effects of Carbon Emissions and Carbon Disclosures

Ella Mae Matsumura

University of Wisconsin–Madison

Rachna Prakash

The University of Mississippi

Sandra C. Vera-Muñoz

University of Notre Dame

ABSTRACT: Using hand-collected carbon emissions data for 2006 to 2008 that were voluntarily disclosed to the Carbon Disclosure Project by S&P 500 firms, we examine the effects on firm value of carbon emissions and of the act of voluntarily disclosing carbon emissions. Correcting for self-selection bias from managers' decisions to disclose carbon emissions, we find that, on average, for every additional thousand metric tons of carbon emissions, firm value decreases by \$212,000, where the median emissions for the disclosing firms in our sample are 1.07 million metric tons. We also examine the firm-value effects of managers' decisions to disclose carbon emissions. We find that the median value of firms that disclose their carbon emissions is about \$2.3 billion higher than that of comparable non-disclosing firms. Our results indicate that the markets

The authors gratefully acknowledge the insightful comments of John Harry Evans III (senior editor), two anonymous reviewers, Bill Baber, Brad Badertscher, Massimiliano Bonacchi, Jeff Burks, Eugene Canjels, Xia Chen, Tom Eggert, Jim Irving, Bjorn Jorgensen, Bill Kinney, Stephannie Larocque, Oliver Zhen Li, Dawn Matsumoto, Fred Mittelstaedt, Bill Nichols, Elizabeth Odders-White, Mina Pizzini, Dave Ricchiute, Tatiana Sandino, Jason Schloetzer, Lloyd Tanlu, Naomi Soderstrom, Anne Terlaak, David Tsui, Linda Vincent, Dan Wangerin, Terry Warfield, workshop participants at Colorado State University, George Mason University, Rochester Institute of Technology, Tel Aviv University, University of Southern California, The University of Texas at El Paso, Darden School of Business-University of Virginia, University of Washington, University of Wisconsin–Madison Department of Accounting Brownbag, the Wisconsin School of Business Rays of Research Seminar, the 2010 Global Accounting and Organizational Change Conference, the 2010 JAAF/KPMG Foundation Conference, the 2011 AAA/MAS Research Conference, the 2011 AAA/FARS Conference, the invaluable research assistance of Luke Derheimer, Manisha Goswami, Megan Hastings, Amie Yuran Li, Meng Li, Hang Zhang, and Jie Zhu, the database procurement support of Stephen Hayes, and the editorial assistance of Dorothy Brady. Also, we appreciate helpful insights from Liz Logan and Maria Moat, partners at PricewaterhouseCoopers LLP, and from Ricky Ashenfelter and Jarrett Jackson, consultants at ClearCarbon by Deloitte. We gratefully acknowledge Maia Kutner and the Carbon Disclosure Project and KLD Research & Analytics, Inc. for making their data available to us. The authors gratefully acknowledge financial support by KPMG's Global Valuation Institute. Professor Prakash gratefully acknowledges financial support from the CFMP at Georgetown University, and the FEAD grant from Rochester Institute of Technology. Professor Vera-Muñoz gratefully acknowledges financial support by KPMG through the Department of Accountancy and the Business Information Center, University of Notre Dame.

Editor's note: Accepted by John Harry Evans III.

Submitted: May 2011

Accepted: July 2013

Published Online: October 2013

penalize all firms for their carbon emissions, but a further penalty is imposed on firms that do not disclose emissions information. The results are consistent with the argument that capital markets impound both carbon emissions and the act of voluntary disclosure of this information in firm valuations.

Keywords: *GHG emissions; voluntary disclosures; firm value.*

JEL Classifications: *G14; Q51; M14.*

Data Availability: *Data are available from the sources identified in the study.*

I. INTRODUCTION

Interest in climate-change risk from institutional investors and various other stakeholders has grown 18-fold in the past decade (PricewaterhouseCoopers 2012, 6) and some sell-side analysts are integrating the financial implications of carbon emissions into their investment recommendations (Eccles, Krzus, and Serafeim 2011). Some observers predict that concern about the relationship between carbon or greenhouse gas (GHG) emissions and global climate change will drive a redistribution of value from firms that do not control carbon emissions successfully to firms that do (GS Sustain 2009, 1). Despite this heightened interest, there is little research regarding the association between carbon emissions, their disclosures, and firm value. Motivated by concern about climate-change risk and carbon emission levels as expressed by investors, regulators, standard-setters, and other stakeholders, we estimate the effects on firm value of carbon emissions and of the *act* of voluntarily disclosing carbon emissions.

Our inquiry is important because recent major initiatives exert pressure on U.S. and non-U.S. firms to increase transparency through disclosures of new nonfinancial climate change and environmental information, including carbon emissions. These initiatives stem from organizations such as the Carbon Disclosure Project (CDP), Ceres, the Global Reporting Initiative (GRI), the Investor Network on Climate Risk (INCR), and the International Integrated Reporting Committee (IIRC).¹ Thus, corporate managers also face growing shareholder pressure to evaluate and report on the risks and opportunities their companies face with respect to climate change, including the exposure of their firms to regulatory and market influences. Notably, climate-change-related shareholder resolutions, as a percentage of all shareholder resolutions, grew from 14 percent in 2004 to 27 percent in 2009 (Ernst & Young 2011).

A firm's enhanced reputation for environmental responsibility, such as investing in renewable energy alternatives that reduce carbon emissions, can potentially bring economic benefits from the broader stakeholder community. These benefits include increased revenues; positive perceptions of employees, customers, suppliers, and other stakeholders who identify the corporation with its environmentally friendly side (Simnett, Nugent, and Huggins 2009a), a more talented and committed work force (Castelo Branco and Lima Rodrigues 2006), and fewer potential claimants on the firm's rents through fines or other compliance costs (Sharfman and Fernando 2008). Collectively, pressure from shareholders and outside organizations creates an impetus for internal management control systems to collect and analyze climate-change-related information, to disclose

¹ The CDP, an independent not-for-profit organization acting on behalf of hundreds of institutional investors, holds the largest repository of carbon emissions information. Ceres is a national network of investors, environmental organizations, and other public interest groups working with companies and investors to address sustainability challenges such as global climate change. The Global Reporting Initiative (GRI) developed what is now the most widely used sustainability reporting framework around the world. The IIRC brings together a cross-section of representatives from the corporate, accounting, securities, regulatory, NGOs, and standard-setting sectors responsible for individual elements of reporting.

it, and to understand the financial consequences of decisions related to climate change as well as broader sustainability issues (Institute of Chartered Accountants in England and Wales 2004).² Thus, even though there are no explicit costs to firms for emitting GHG in the U.S., evidence on the extent to which capital markets incorporate carbon emissions into firm valuation will help U.S. firms make important decisions regarding the broad cost-benefit trade-offs of allocating resources to carbon emissions-reduction initiatives (Thaler and Sunstein 2009).³

Our study also lends insight into matters addressed by U.S. and international accounting standard-setters and regulators. For example, in January 2010, the SEC responded to investors' concerns about required climate-change-related disclosures by providing explicit guidance on disclosures of risks and opportunities related to climate change in SEC regulatory filings. In conjunction with this guidance, SEC Commissioner Elisse Walter expressed concern that "many public companies are in fact providing disclosure about significant climate-change-related matters through mechanisms outside of the disclosure documents they file with the Commission."⁴ Indeed, the International Auditing and Assurance Standards Board (IAASB) recently approved ISAE 3410, Assurance Engagements on Greenhouse Gas Statements, which provides guidance for independent assurance on GHG statements issued by U.S. and international firms (International Auditing and Assurance Standards Board [IAASB] 2012).⁵ According to the IAASB, the independent assurance can be provided to report on an entity's GHG statement prepared: "(a) As part of a regulatory disclosure regime; (b) As part of an emissions trading scheme; or (c) To inform investors and others on a voluntary basis" (IAASB 2012, para. 1).

Extant accounting research reports that capital markets use environmental disclosure/liability information in assessing how well firms manage exposure to environmental risk (Barth and McNichols 1994; Blacconiere and Patten 1994; Cormier and Magnan 1997; Campbell, Sefcik, and Soderstrom 1998). These papers examine environmental disclosures mandated by government agencies such as the U.S. Environmental Protection Agency (EPA) and the Canadian Environment Ministries, or by financial reporting regulations, and the disclosures pertain to toxic substances with well-identified risks. In contrast, carbon emissions have ill-defined risks; further, disclosure of carbon emissions is voluntary. Other related research examines the market-value relevance of excess emissions allowances within the context of the enacted sulfur dioxide (SO₂) emissions trading scheme (ETS) in the U.S. (Hughes 2000; Johnston, Sefcik, and Soderstrom 2008).⁶ SO₂ emissions measurement involves highly accurate continuous emissions monitoring (CEM) systems, reporting of these emissions in the U.S. is mandatory, and SO₂ emission allowances are traded in the market. It follows that, unlike the measurement uncertainty inherent in carbon emissions,⁷ there

² Emissions from various greenhouse gases are expressed in carbon-dioxide equivalents (CO₂-e) based on their global warming potential. The Environmental Protection Agency (EPA) has concluded that the more carbon emissions there are in the atmosphere, the more heat is trapped. This leads to rising temperatures and, thus, climate change. Source: <http://www.epa.gov/climatechange>

³ For example, Microsoft recently announced that it has established an internal "price" for carbon as part of its drive to become carbon-neutral (Vagus 2012). The company has determined its own carbon price based on the cost of buying offsets, whereby the entity pays for decreasing another entity's emissions or increasing another entity's removals, compared to a hypothetical baseline, and the price of investing in renewable energy.

⁴ Commissioner Walter's full speech is available at: <http://www.sec.gov/news/speech/2010/spch012710ebw-climate.htm>

⁵ The standard, approved in March 2012, is effective for assurance reports covering periods ending on or after September 30, 2013.

⁶ The EPA states, "The Clean Air Act requires the EPA to set national ambient air quality standards (NAAQS) for sulfur dioxide and five other pollutants considered harmful to public health and the environment (the other pollutants are ozone, particulate matter, nitrogen dioxide, carbon monoxide, and lead)." Source: http://www.epa.gov/ttn/naaqs/standards/so2/s_so2_index.html

⁷ Measurement of carbon emissions often involves estimation. The EPA provides guidance for such estimation at <http://www.epa.gov/climateleadership>

is little uncertainty about the nature, measurement, and magnitude of the cost of SO₂ emissions. Furthermore, findings in Hughes (2000) and Johnston et al. (2008) are based on small samples and/or a few industries.

Unlike toxic emissions, during the period of our study carbon emissions were largely unregulated in the U.S. and firms were not required to disclose them. Accounting research on the market-value relevance of voluntarily disclosed carbon emissions is rare. Using a sample of 58 publicly traded Australian firms expected to be affected by a proposed national ETS, Chapple, Clarkson, and Gold (2013) find that the markets penalize firms that will be affected by the proposed ETS. However, they only study firms that disclose emissions and do not address how the capital markets integrate both carbon emissions and the act of voluntary disclosure of this information into firm valuations.

Our study adds to extant accounting research by examining the effect of carbon emissions on firm value after correcting for self-selection bias, as well as the effect on firm value of the act of voluntarily disclosing carbon emissions. We hand-collect carbon emissions data for 2006 to 2008 for S&P 500 firms to investigate the hypothesis that firm value is negatively associated with carbon emissions. We obtain all carbon emissions data publicly available from CDP, about 40 percent of our sample. We also divide our sample based on whether firms will be subject to the EPA's GHG Mandatory Reporting Rule (Environmental Protection Agency [EPA] 2009).⁸ Because firms in our sample period choose to disclose their carbon emissions, we correct for self-selection bias by incorporating systematic firm- and industry-level characteristics in our analyses.

We find that, on average, for each additional thousand metric tons of carbon emissions for our sample of S&P 500 firms, firm value decreases by \$212,000.⁹ This translates into a firm-value penalty of \$1.4 billion for firms in the third quartile in terms of carbon emissions relative to firms in the first quartile.¹⁰

We conduct several sensitivity tests to assess the robustness of our main results and our inferences are unchanged. These tests include estimating the effects of carbon emissions on firm value using the Ohlson (1995) model, and estimating the relationship between changes in firm value and changes in carbon emissions. In two additional tests, we use December fiscal year-end firms only,¹¹ and also control for the effects of other unbooked liabilities to rule them out as alternative explanations for our main results.

Our finding of a negative association between carbon emissions and firm value begs the question, "If the capital markets penalize firms for their carbon emissions, then why do firms choose to disclose information on them?" We argue that managers weigh the costs and benefits of disclosing carbon emissions and choose to disclose only when the perceived benefits of doing so outweigh the perceived costs. Therefore, we also examine the firm-value effects of the act of voluntarily disclosing carbon emissions. Using propensity score matching (Rosenbaum 2005) and doubly robust regression (Imbens and Wooldridge 2007), we find that the median market value of firms that disclose their carbon emissions is about \$2.3 billion higher than that of their non-disclosing counterparts. The median market value of the sample disclosing firms is

⁸ The EPA's Greenhouse Gas Mandatory Reporting Rule requires fossil fuel and industrial gas suppliers, direct GHG emitters, and manufacturers of heavy-duty and off-road vehicles and engines to report their GHG emissions to the EPA, beginning with carbon emissions for 2010.

⁹ To provide some perspective on this number, we note that the net present value of \$17 per metric ton of carbon emissions discounted in perpetuity at an interest rate of 8 percent is approximately \$212 (that is, \$212,000 per thousand metric tons of carbon emissions).

¹⁰ The median emissions for our sample firms are 1.07 million metric tons.

¹¹ The carbon emissions data are for calendar year *t*, while our accounting data are for fiscal year *t*. A majority of our sample firms have a December fiscal year-end, so to minimize the effects of different fiscal and calendar year-ends, we also conduct analyses including only firms that have a December fiscal year-end.

approximately \$16 billion. These results, combined with our main result of a negative association between carbon emissions and firm value, are consistent with the argument that capital markets impound both carbon emissions and the act of voluntary disclosure of this information in firm valuations. While all firms are penalized for their carbon emissions regardless of whether they disclose them, firms that do not disclose their carbon emissions face a further capital market penalty.

Our study contributes to the accounting literature in four distinct and important ways. First, extant accounting research examines nonfinancial mandated environmental disclosures involving relative certainty regarding the nature, measurement, and magnitude of the cost of the emissions. Examples include studies of SO₂ emissions by Hughes (2000) and Johnston et al. (2008), or financial accounting information associated with measurement uncertainty that affects estimation of reported accounting numbers for Superfund liabilities (Campbell, Sefcik, and Soderstrom 2003). In contrast, we focus on carbon emissions, which are largely unregulated in the U.S. but involve significant climate-change risk.¹² There is considerable uncertainty about the measurement and reporting of carbon emissions, which are currently disclosed at management's discretion in the U.S. Our study provides empirical evidence concerning the extent to which investors incorporate often unassured, uncertain nonfinancial information in their firm-value assessments. Further, our study provides evidence on the extent to which the markets use environmental information from sources other than regulatory agencies for firm valuation.

Second, we examine the relation between carbon emissions and firm value for disclosing and non-disclosing firms, correcting for self-selection bias associated with managers' decisions to disclose carbon emissions. To our knowledge, this is the first study that does so. Third, our paper is the first to investigate the relation between the magnitude of carbon emissions and firm value and estimate the price that U.S. capital markets are imputing to carbon emissions. Fourth, our study provides empirical evidence of a firm-value penalty for non-disclosing firms. To our knowledge, this is the first study to provide empirical evidence on the firm-value effects of *both* the decision to voluntarily disclose carbon emissions *and* the magnitude of carbon emissions.

We next review the literature and develop our hypothesis. Sections III and IV present our research design and results, respectively, and Section V concludes the paper.

II. INSTITUTIONAL CONTEXT, THEORY, AND HYPOTHESIS

U.S. firms face increasing pressure from various stakeholders, including investors, financial risk managers, insurance companies, carbon traders, and NGOs, to measure, disclose, monitor, and manage their carbon emissions (Fornaro, Winkelman, and Glodstein 2009). Other potential carbon-related costs include capital expenditures resulting from environmental initiatives, such as acquiring or developing less carbon-intensive technologies and processes, research and development to develop goods and services associated with lower carbon emissions, and other corporate initiatives such as reducing employees' carbon footprint. Carbon emissions have also become an essential element in analyzing a company's risk profile, potential "unbooked" liabilities and costs without lasting economic benefits (such as fines, penalties, and awards from lawsuits), and firms' financial performance. For instance, Standard & Poor's downgraded the debt of a large U.K. power-generating company, Drax, owing in part to future business risks from new European emissions trading rules that are expected to increase carbon costs (Barley 2009).

¹² Climate-change risk encompasses risks driven by: changes in regulation, changes in physical climate parameters, and changes in other climate-related developments (see <https://www.cdproject.net/CDP%20Questionnaire%20Documents/Investor-CDP-2013-Information-Request.pdf>).

Some stakeholders advocate a tax on carbon emissions or an emissions allowance system. To put such potential direct costs in perspective, on average, S&P 500 firms emit 382 tons of carbon dioxide-equivalent, CO₂-e, for every U.S. \$1 million of revenue that they generate (Investor Responsibility Research Center Institute [IRRCi]/Trucost Report 2009). If the Report's suggested market price of \$28.24 were applied to each ton of CO₂-e emitted by the S&P 500 firms, then carbon emission costs would total over \$92.8 billion, or 1.08 percent of revenue from the companies in 2007, and 5.5 percent of earnings before interest, tax, depreciation, and amortization (IRRCi/Trucost Report 2009).

Theory and Hypothesis

Accounting research on the value relevance of firms' environmental disclosures falls into three broad categories. The first includes studies examining the market valuation of environmental disclosures that are mandated either by accounting standards (Barth and McNichols [1994], Blacconiere and Patten [1994], and Campbell et al. [1998] on Superfund liabilities), or by the government (Hughes [2000] and Johnston et al. [2008] on sulfur dioxide [SO₂] emissions; Cormier and Magnan [1997] on water pollution; and Connors, Johnston, and Silva-Gao [2013] on TRI valuation).¹³ The first subset of these studies focuses on the extent to which the markets view as adequate the amounts recognized by firms for Superfund-related liabilities.

The second subset in the first category examines the market-value relevance of excess emission allowances for the SO₂ ETS in the U.S. (Hughes 2000; Johnston et al. 2008). Hughes (2000) examines the firm-value effects of SO₂ emissions using a sample of publicly traded electric utilities targeted as high-polluting by the 1990 Clean Air Act Amendments (CAAA). He finds that the market penalizes the high-polluting utilities during 1989 to 1991, the years that are most likely associated with new CAAA compliance costs. Johnston et al. (2008) extends Hughes (2000) by examining the value relevance of SO₂ emission allowances held by publicly traded U.S. electric utilities, which are subject to the 1990 CAAA ETS. The authors find support for their reasoning that emission allowances above a firm's current needs have two components—an asset value and a real option value—that will be valued by the market.

Unlike the institutional context of our study in which carbon emissions are measured or estimated and disclosed voluntarily, SO₂ emissions are measured using highly accurate CEM systems, reporting of SO₂ emissions in the U.S. is mandatory, and SO₂ emission allowances are traded in the market. It follows that there is relative certainty about the nature, measurement, and magnitude of the cost of SO₂ emissions. Although the findings in Hughes (2000) and Johnston et al. (2008) are based on small samples and/or a few industries, the results of these studies are consistent with a negative association between electric utilities' SO₂ emissions and market value.

The second broad category examines the market valuation of environmental capital expenditures (Clarkson, Li, and Richardson 2004; Cho, Freedman, and Patten 2012). Using a sample from the pulp and paper industry for 1989 to 2000, Clarkson et al. (2004) examine the market valuation of environmental capital expenditures conditional on the firms' environmental performance. They predict and find that the market places a positive value on environmental expenditures for low-polluting firms, but assigns no value to the environmental expenditures of high-polluting firms. Further, they find that investors use disclosures of environmental capital expenditures to assess unbooked environmental liabilities for high-polluting firms. More recently, Cho et al. (2012) use a sample of *Fortune* 500 firms operating in industries subject to the EPA's

¹³ The EPA collects data annually from facilities that release or transfer certain toxic chemicals. The Toxics Release Inventory (TRI) data are available on the EPA website (<http://www.epa.gov/tri/triprogram/whatis.htm>).

TRI program, and find that companies use the disclosure of environmental capital spending as a strategic tool to address their exposure to political and regulatory concerns.

The third broad category examines more recent research on the market valuation of *voluntarily* disclosed carbon emissions. Chapple et al. (2013) examine the association between a dichotomous measure of high- and low-carbon emissions intensity and the market value of equity for a sample of 58 publicly traded Australian firms originally subject to a proposed national ETS. Using a modified Ohlson (1995) valuation model, the study finds that carbon-intense firms suffer a penalty of 6.57 percent of market capitalization. However, the study does not account for the voluntary nature of the firms' carbon emission disclosures or for potential self-selection bias during the sample period of 2006 to 2009. Further, unlike our sample of S&P 500 firms, the more carbon-intensive firms in their sample are generally smaller, less profitable, and riskier than their less carbon-intensive counterparts.¹⁴

Walley and Whitehead (1994, 47) argue that "[i]n a world where [managers] cannot do everything, only a value-based approach allows informed trade-offs between [environmental] costs and benefits." Our research addresses this point by examining the effect of carbon emissions on firm value after correcting for self-selection bias associated with managers' decisions to voluntarily disclose carbon emissions. Moreover, we estimate the effect on firm value of disclosing carbon emissions.

We draw on value relevance research (Barth, Beaver, and Landsman 2001; Holthausen and Watts 2001) as a theoretical framework for assessing whether a nonfinancial environmental performance metric in the form of carbon emission levels provides information that investors use for firm valuation. Based on this research, we posit that if capital markets believe that carbon emissions are relevant for valuation and are measured reliably enough, carbon emission levels will have significant market-value implications (Barth et al. 2001).

Although carbon emissions information is self-reported and frequently unassured, it may nevertheless be reasonably credible. First, the markets can assess the credibility of the data by comparing them to similar data from other firms in the same industry, and some of the data may be assured. Some firms operate in jurisdictions such as the European Union, where emissions are regulated. Second, although responding to the CDP questionnaire is voluntary, once a firm decides to participate, it is significantly more likely to participate in the future (Stanny 2013). These repeated interactions between the CDP and the firm will generally increase the cost of reporting untruthfully, particularly as more firms in the industry decide to report and assurance of emissions becomes more widespread. Untruthful reporting that is eventually revealed can damage the firm's overall reporting credibility and expose it to litigation risk.

Market-value penalties associated with carbon emissions reflect first, the perceived relationship between carbon emission levels and the firms' climate change related risk profile. This risk is driven by climate change regulation, uncertainty surrounding new regulatory compliance, and uncertainty surrounding physical climate parameters, such as severe weather (Epstein 2008, 62). The second source of market-value penalty is the cost of measuring, disclosing, monitoring, and reducing carbon emissions. According to natural-resource-based theory (Hart 1995), a firm's key resources and capabilities affect its ability to sustain its competitive advantage. This argument points to the importance, for firm valuation, of cost-benefit trade-offs of allocating resources to carbon emissions reduction initiatives (Thaler and Sunstein 2009). It follows that firms that do not integrate climate change risk into their business strategy (e.g., by investing in renewable energy alternatives that reduce carbon emissions) are likely to lower investors' market-value expectations relative to firms

¹⁴ Finally, a few other studies examine factors that affect firms' decisions to adopt proactive environmental strategies (Clarkson, Li, Richardson, and Vasvari 2011), and whether corporate social performance enhances financial performance (Dhaliwal, Li, Tsang, and Yang 2011; also, see a literature review by Clarkson [2012]).

that do (Hart 1995; Epstein 2008, 145). Consistent with these arguments, we propose the following hypothesis (in alternative form) regarding the firm-value effect of carbon emissions:

H1: Firm value is negatively associated with carbon emissions.

There are at least two reasons why our hypothesis may not obtain. First, carbon emissions meet the definition of an externality. There is currently a great deal of uncertainty regarding the extent to which U.S. firms will be required to internalize the cost of their carbon emissions in the future. In turn, the market is likely to reflect such uncertainty in valuing future liabilities from carbon emissions. Also, firms may be able to pass along the cost of their carbon emissions to their consumers and/or supply chain partners (Initiative for Global and Environmental Leadership [IGEL]/Knowledge@Wharton 2012), thus reducing the firms' share of the costs of carbon emissions.¹⁵ Second, if the capital markets view voluntarily disclosed carbon emissions as insufficiently reliable (Barth et al. 2001), then they may disregard this information when making firm-value assessments (Simnett, Vanstraelen, and Chua 2009b). Despite these considerations, the results of Hughes (2000), Johnston et al. (2008), and Chapple et al. (2013), as discussed above, lead us to predict a negative association between carbon emissions and firm value.

Firm-Value Effects of Decision to Disclose Carbon Emissions

If the capital markets penalize firms for their carbon emissions, then why do firms choose to disclose them? In general, disclosures provide benefits through reduced information asymmetry between the firm and outsiders, including its investors, thus facilitating efficient allocation of scarce resources (Healy and Palepu 2001). Firms making truthful voluntary carbon emission disclosures deliver transparent nonfinancial information to investors that informs them of future costs that may be imposed upon the firm due to its carbon emissions. If firms do not disclose carbon emissions, then investors will not only impute the firms' carbon emissions, but also likely treat non-disclosure as an adverse signal and penalize non-disclosing firms (Milgrom 1981). Also, investors are likely to undertake costly information searches regarding the non-disclosers' emissions, thus increasing costs to investors and, ultimately, the firms' costs (Johnston 2005). Research on voluntary corporate social responsibility (CSR) disclosures documents that firms that issue CSR reports experience a decrease in their cost of capital if the firms show superior CSR performance (Dhaliwal et al. 2011).¹⁶ Voluntary disclosures are also used to reduce potential regulatory intervention (Blacconiere and Patten 1994).

Furthermore, the act of disclosure itself, even in the absence of mandated behavioral changes, can be associated with beneficial consequences from the investors' viewpoint. For example, an unintended benefit of the TRI disclosure rule under the Emergency Planning and Community Right to Know Act was a large reduction in toxic emissions in the U.S. (Thaler and Sunstein 2009). Environmentally concerned groups tend to target the worst TRI offenders, thus producing an "environmental blacklist." Consistent with Thaler and Sunstein's (2009) concept of "social nudge," firms seek to avoid appearing on such a list because the ensuing bad publicity could lower the company's market value. Therefore, companies that end up on the list are motivated to take steps to

¹⁵ Nevertheless, firms with lower carbon costs will have a competitive advantage through lower total costs, *ceteris paribus*.

¹⁶ Although both Dhaliwal et al. (2011) and our paper include an examination of voluntary disclosures, we also analyze the firm-value effect of the magnitude of carbon emissions. This information is much more precise with respect to carbon emissions than the binary environmental performance measures of strengths and weaknesses on particular components used in Dhaliwal et al. (2011). Further, their paper describes only rewards (in the form of lower cost of equity) for disclosing positive news (CSR reports), and does not discuss the cost of disclosing *per se*.

reduce their TRI in order to be removed from the list, and the companies not yet on the list are motivated because they want to ensure that they do not end up there. Although carbon disclosures are voluntary, firms face considerable pressure to disclose their emissions. Hence, similar motivations exist for disclosers to reduce their carbon emissions. Moreover, a firm that discloses its carbon emissions signals its ability to measure its emissions, a prerequisite for managing them. This discussion suggests that the markets will reward the firms that disclose their carbon emissions.

Moser and Martin (2012, 804) argue that viewing CSR activities and related disclosures more broadly as being motivated by shareholders and non-shareholders alike raises issues that do not fall within the traditional perspective of shareholder value maximization. This argument implies that it is likely that allocation of scarce corporate resources to some CSR investments, such as measuring and disclosing carbon emissions, is made *at the expense* of shareholders. For instance, carbon emission disclosures can impose proprietary costs on some firms (Li, Richardson, and Thornton 1997). Also, government regulators such as the EPA can use disclosures by high-carbon-emitting firms as grounds for investigations that can ultimately increase those firms' compliance costs. Furthermore, carbon emissions disclosures could invite costly litigation by previously uninformed victims of GHG-related climate change, benefit competitors' green-marketing strategies aimed at environmentally conscious consumers, and provide ammunition for public interest groups (e.g., Ceres) to press for stricter regulation. In summary, under some circumstances the markets will penalize firms that voluntarily disclose their carbon emissions.

Because the answer to the question, "Why do firms choose to disclose their carbon emissions?" is not self-evident in the context of this study, we address the question by examining the firm-value effects of the act of voluntarily disclosing carbon emissions. By investigating whether the markets reward or penalize voluntary disclosers of carbon emissions, we provide insights on the two perspectives discussed above.

III. RESEARCH DESIGN

Sample and Data

Our sample consists of all S&P 500 firms for the three-year period 2006 to 2008. We choose 2006 as the initial year because the S&P 500 firms were first included as a group in the 2007 CDP report, which provides 2006 data. We choose 2008 as our final year because in April 2009 the EPA issued a proposed rule on GHG mandatory reporting that became effective on December 29, 2009 requiring reporting of carbon emissions for 2010 and thereafter (EPA 2009).¹⁷ To avoid confounding voluntary disclosure to the CDP with the transition to the EPA's mandatory reporting rule, we exclude 2009 data. In order to maintain a constant sample over this period we use firms that were included in the S&P 500 index on December 31, 2007.

We hand-collect carbon emissions data from 2006 to 2008 from the CDP database.¹⁸ The CDP requests information from the world's largest companies as measured by market capitalization. Currently, the CDP acts on behalf of 665 institutional investors representing more than \$78 trillion

¹⁷ The original reporting deadline of March 30, 2011 for 2010 was extended to September 30, 2011.

¹⁸ This entailed manually collecting and cross-validating carbon emissions numbers from data purchased from the CDP with *individual* firm responses available at the CDP website for all S&P 500 firms for our three-year period. We found numerous discrepancies between the data in the individual firm responses and the CDP's annual summary reports on S&P 500 firms, usually because the summaries only report data available up to the point when the summaries were prepared. For example, the summary report for a given year could show a firm as "not responding," while the CDP website would show the firm as responding in that year. These discrepancies imply that caution should be used in relying exclusively on the CDP annual summary reports.

in assets under management.¹⁹ The number of firms answering the CDP survey increased from 235 in 2003 to more than 3,000 companies in 2011, including 81 percent of the Global 500 firms and 68 percent of the S&P 500 firms. PricewaterhouseCoopers LLP has been the global sponsor of the CDP since 2008, with responsibility for analyzing the survey responses for the S&P 500, Global 500, and FTSE 600 companies.²⁰

The CDP questionnaire elicits information on carbon emissions in metric tons, energy, and trading.²¹ Participation in the CDP questionnaire is voluntary and different sets of firms (1) do not respond or respond indicating their decision to decline participation; (2) provide partial information, such as links to information generally available at the firm's website, for instance their CSR reports, without answering the questionnaire; (3) respond to the questionnaire but allow the CDP to make the responses available only to institutional investors who are signatories of the CDP; and (4) respond to the questionnaire and allow the CDP to make the responses publicly available.

Table 1 provides the S&P 500 firms' responses to the CDP questionnaire and carbon emissions for the years 2006 to 2008. Of the total sample, 550 firm-years (38.12 percent) reflect responses that firms allow to be publicly available, and 184 firm-years (12.75 percent) reflect responses by firms that do not allow their responses to be made public. Further, 205 firm-years (14.21 percent) come from firms that provide only partial information to the CDP. As explained in Table 1, footnote c, for 35 firm-years we were able to obtain carbon emissions data from the firms' responses to the CDP questionnaire in a subsequent year. In total, we obtained carbon emissions data for 584 firm-years of 1,443 firm-year observations, or 40.47 percent, representing 256 firms. The disclosures per year are as follows (untabulated): 162 (28 percent) are from 2006, 202 (35 percent) are from 2007, and 220 (37 percent) are from 2008.

Of the original 1,443 firm-year observations in Table 1, we lose 78 for which we are unable to obtain information necessary to run the disclosure-choice model, and 815 observations for which carbon emissions data are not available (Table 2). Therefore, we estimate the firm-value model in Equation (1) below with the remaining 550 firm-year observations.²²

We also collect environmental performance data from KLD STATS, a database that includes annual snapshots of the environmental, social, and governance (ESG) performance of companies rated by KLD Research & Analytics, Inc. Each year, KLD freezes its ratings to reflect the firm status at calendar year-end. KLD STATS provides a binary summary of KLD's ratings based on each firm's environmentally proactive and damaging activities. There are six proactive dimensions, including recycling and clean energy, and seven damaging dimensions, including substantial fines

¹⁹ See <https://www.cdproject.net/en-US/Pages/About-Us.aspx>. Institutional investors who sign the CDP questionnaire are known as "CDP signatories."

²⁰ See <http://www.pwc.com/gx/en/sustainability/publications/carbon-disclosure-project/about.jhtml>. We explored other potential sources of carbon emissions data, including the Chicago Climate Exchange (CCX), 10-K reports filed with the SEC, CSR reports, the Pew Center on Global Climate Change, and state and regional reporting initiatives. However, we deemed these sources unsuitable for our study because they do not uniformly report carbon emissions data, or provide only limited data that include very few S&P 500 firms, or report data by facility or state, but not by firm.

²¹ Firms must respond to this section using the GHG Protocol Corporate Standard (Revised Edition), available at: <http://www.ghgprotocol.org/standards/corporate-standard>. Firms report their global carbon emissions broken down by Scope 1 (direct emissions from GHG sources owned or controlled by the firm), Scope 2 (indirect emissions caused by the firm's consumption of electricity, heat, cooling or steam brought into its reporting boundary), and Scope 3 (emissions from employee business travel, external distribution/logistics, disposal of the company's products and services, and the company's supply chain). A number of firms in our sample do not provide carbon emissions broken down into scopes.

²² Of the firms that provide carbon emissions information, the two industries with the largest number of observations in our sample are chemical manufacturing and utilities, with 71 firm-years each (untabulated).

TABLE 1
S&P 500 Firms' Responses to CDP Questionnaire and Carbon Emissions Information for Years 2006 to 2008

	Carbon Emissions Available?					
	No	Percent	Yes	Percent	Total	Percent
Responded to CDP Questionnaire? ^a						
Response publicly available	1 ^b	0.07	549	38.04	550	38.11
Response not publicly available	183	12.68	1 ^c	0.07	184	12.75
Provided partial information	201	13.93	4 ^c	0.28	205	14.21
No/Declined to participate	474	32.85	30 ^c	2.08	504	34.93
Total firm-year observations	859	59.53	584	40.47	1,443	100.00
Less: Observations with missing information for disclosure-choice model	44		34		78	
Final sample of firm-year observations	815	59.71	550	40.29	1,365	100.00

^a The carbon emissions data are collected annually by the CDP on behalf of institutional investors, purchasing organizations, and government bodies. Participation in the CDP questionnaire is voluntary. Therefore, firms may choose to: not respond to the questionnaire/decline to participate; provide partial information (e.g., provide links to information generally available on the firm's website, such as their corporate social responsibility reports) without answering the questionnaire; respond to the questionnaire but not allow the CDP to make the responses publicly available; or respond to the questionnaire and allow CDP to make the responses publicly available.

^b One firm that responded to the CDP questionnaire and made its responses publicly available did not provide carbon emissions.

^c These firms declined to participate in the CDP questionnaire, or did not allow the CDP to make their responses publicly available, or provided only partial information in one year; however, we were able to obtain their carbon emissions data from the firms' responses to the CDP questionnaire in a subsequent year.

or penalties paid for violations of environmental regulations.²³ In each case, if KLD identifies a proactive initiative or a damaging action in a particular dimension, then it indicates this with a 1, otherwise the dimension is coded 0.

Empirical Models

Firm-Value Effects of Carbon Emissions

We examine the firm-value effect of carbon emissions using the balance sheet valuation model commonly used in prior literature such as Barth and McNichols (1994) and Campbell et al. (2003). Because managers *choose* to disclose their carbon emissions to the CDP and the public, our sample may be systematically biased, that is, suffer from self-selection bias. We correct for this by jointly estimating the decision to disclose carbon emissions (discussed in the next subsection) and the effect of carbon emissions on firm value (Heckman 1979; Maddala 1983). Equation (1) shows the firm-value model:

$$MKT_t = \beta_0 + \beta_1 TCO2_t + \beta_2 ASSET_t + \beta_3 LIAB_t + \beta_4 OPINC_t + \varepsilon_t, \tag{1}$$

where our proxy for firm value, MKT_t , is the market value of common equity (in millions of dollars), calculated as the number of shares outstanding multiplied by the price per share of the firm's common stock at the end of calendar year t . Although the emissions data for the current year

²³ For further details, see the KLD Manual at WRDS: <https://wrds-web.wharton.upenn.edu/wrds>

TABLE 2
Descriptive Statistics

Panel A: Full Sample and Breakdown of Sample by EPA Group

Variable	Full Sample					EPA = 1		EPA = 0		t-stat	Wilcoxon
	Mean	Q1	Median	Q3	Std. Dev.	Mean	Median	Mean	Median	p-value	p-value
Firm-Value Model (n = 550) ^a											
						n = 229		n = 321			
MKT	33,111.41	7,718.98	16,013.08	33,403.01	45,718.14	31,926.01	16,470.81	33,957.06	15,297.94	0.69	0.36
TCO2	11,455.41	262.66	1,068.10	6,849.44	26,712.11	23,741.68	6,467.23	2,690.44	484.67	0.00	0.00
ASSET	59,940.55	9,176.52	19,415.11	40,519.00	148,840.00	35,161.54	22,970.00	77,617.79	17,750.87	0.00	0.31
LIAB	46,803.25	5,054.00	12,330.00	25,288.00	137,389.90	22,326.47	14,437.00	64,264.88	10,342.00	0.00	0.11
OPINC	3,880.59	795.00	1,616.05	3,618.00	6,754.85	3,876.16	1,710.00	3,883.74	1,563.00	0.50	0.22
Disclosure-Choice Model (n = 1,365) ^a											
						n = 401		n = 964			
CNCRN	0.67	0.00	0.00	1.00	1.15	1.54	1.00	0.31	0.00	0.00	0.00
STRNG	0.53	0.00	0.00	1.00	0.92	0.89	1.00	0.38	0.00	0.00	0.00
PROPDISCL	39.81	25.00	40.00	57.14	24.19	54.89	57.14	33.54	34.78	0.00	0.00
SIZE	9.55	8.57	9.41	10.32	1.37	9.56	9.56	9.55	9.36	0.47	0.09
MF	4.87	0.00	4.00	8.00	4.72	5.12	5.00	4.77	4.00	0.11	0.21
BM	0.49	0.24	0.39	0.62	0.41	0.44	0.37	0.51	0.39	0.01	0.15
LEV	0.40	0.21	0.38	0.57	0.26	0.42	0.42	0.40	0.36	0.03	0.00
II	80.43	71.78	81.28	89.69	14.42	78.26	79.45	81.33	82.00	0.00	0.00
FRNSALE	25.62	0.00	21.23	44.89	25.04	29.04	30.30	24.20	17.23	0.00	0.00

(continued on next page)

TABLE 2 (continued)

Panel B: Breakdown of Sample by Availability of Carbon Emissions Data

Variable	DISC_CDP = 1		DISC_CDP = 0		t-stat p-value	Wilcoxon p-value
	Mean	Median	Mean	Median		
Firm-Value Model	n = 550		n = 815 ^b			
MKT	33,111.41	16,013.08	14,323.46	8,288.10	0.00	0.00
TCO2	11,455.41	1,068.10	—	—	—	—
ASSET	59,940.55	19,415.11	38,263.42	8,539.00	0.00	0.00
LIAB	46,803.25	12,330.00	32,118.06	5,027.43	0.02	0.00
OPINC	3,880.59	1,616.05	1,921.13	831.00	0.00	0.00
Disclosure-Choice Model						
CNCRN	1.05	0.00	0.41	0.00	0.00	0.00
STRNG	0.96	1.00	0.24	0.00	0.00	0.00
PROPDISCL	54.48	55.26	29.92	30.77	0.00	0.00
SIZE	9.97	9.87	9.27	9.05	0.00	0.00
MF	5.39	5.00	4.53	4.00	0.00	0.37
BM	0.46	0.38	0.51	0.39	0.02	0.01
LEV	0.42	0.40	0.39	0.37	0.03	0.00
II	77.31	77.71	82.53	83.63	0.00	0.00
FRNSALE	32.40	32.07	21.04	13.64	0.00	0.00

^a We estimate the firm-value model using the 550 firm-year observations for which carbon emissions data are available. We estimate the disclosure-choice model using the 1,365 firm-year observations comprising both disclosing and non-disclosing firms.

^b For MKT we have data for only 812 firm-years in the DISC_CDP = 0 group. The three missing firm-year observations are for one firm for which we were not able to obtain both shares outstanding and price data. For variable definitions see Appendix A.

do not become available to the markets until the middle of the following year, we treat subsequent realizations of the emissions as our best estimate of market expectations.²⁴

Our independent variable of interest, *TCO2_t*, denotes carbon emissions in thousands of metric tons. Consistent with our hypothesis of a negative association between carbon emissions and firm value, we expect a negative *TCO2_t* coefficient. Following prior related research (Barth and McNichols 1994; Campbell et al. 2003), our balance sheet valuation model includes total assets (*ASSET_t*) and liabilities (*LIAB_t*) at the end of the fiscal year, with a positive coefficient expected for *ASSET_t* and a negative coefficient expected for *LIAB_t*. Consistent with prior research (Barth and McNichols 1994; Campbell et al. 2003) and Barth and Clinch’s (2009) finding that unscaled market value of equity estimates generally perform better than scaled market value models, we do not scale *MKT_t* or *TCO2_t* in this model. Moreover, the coefficients from an unscaled model are intuitive and economically meaningful (Ziliak and McCloskey 2004).

To control for potential correlated omitted variable bias, we include a proxy for the firm’s operating income in year *t*, denoted as *OPINC_t*. Firms with higher operating income are not only valued more highly by the markets, but they are also better able to invest resources for measuring

²⁴ Although using subsequent realizations gives rise to a look-ahead bias, this is not a concern for our study because we are not proposing trading strategies based on carbon emissions data, but instead are documenting an average firm-value effect. Using end-of-the-year market values is also in line with other literature examining valuation implications of environmental data (Barth and McNichols 1994; Johnston et al. 2008).

and controlling their carbon emissions. It follows that firms with better performance are more likely to have both higher market values and lower emissions. Therefore, we include $OPINC_t$ as an additional control variable in Equation (1) and predict a positive association between $OPINC_t$ and MKT_t . To control for industry-level characteristics, we include industry fixed effects at the two-digit SIC code level in Equation (1). Our financial statement data are from the Compustat database unless otherwise indicated.

Choice to Disclose Carbon Emissions

Firms choose to disclose carbon emissions to the CDP if the perceived benefits of doing so outweigh the perceived costs. Firms are unlikely to disclose their carbon emissions to the CDP if: (1) they have low carbon emissions such that the cost of measuring and collecting this information exceeds the benefits of doing so; (2) the firms have significant carbon emissions but have yet to implement internal measurement systems and processes to collect emissions information; and (3) the firms have a high level of emissions and therefore are reluctant to disclose this “bad news” to investors and other outside stakeholders due to proprietary and other related costs.²⁵ Because managers’ evaluations of the perceived benefits and costs of disclosing are unobservable, we rely on voluntary disclosure theory (Verrecchia 1983; Healy and Palepu 2001) to model managers’ disclosure decisions as a function of various firm- and industry-level characteristics.

To control for self-selection, we jointly estimate the decision to disclose carbon emissions and the effect of carbon emissions on firm value (Equation (1)). Equation (2) shows the logit model we use to examine the disclosure choice:

$$\begin{aligned} DISC\_CDP_t = & \beta_0 + \beta_1 STRNG_t + \beta_2 CNCRN_t + \beta_3 PROPDISCL_t + \beta_4 SIZE_t + \beta_5 MF_t \\ & + \beta_6 BM_t + \beta_7 LEV_t + \beta_8 II_t + \beta_9 FRNSALE_t + \beta_{10} DISC\_CDP_{t-1} + \beta_{11} EPA_t \\ & + \varepsilon_t, \end{aligned} \quad (2)$$

where $DISC\_CDP_t$ is an indicator variable that is coded as 1 if the firm discloses its year t carbon emissions data to the CDP and allows public disclosure by the CDP, and 0 otherwise. We select the independent variables in model (2) based on economic theory (Akerlof 1970; Milgrom 1981) and on prior research on environmental disclosures (Li et al. 1997; Clarkson, Li, Richardson, and Vasvari 2008).

Environmental Performance Variables

Economic theory predicts that firms with good performance (good type) have incentives to separate themselves from firms with poor performance (bad type) to avoid an adverse selection problem. That is, firms voluntarily reveal credible private information to distinguish themselves from the worst performers (Akerlof 1970; Milgrom 1981). Consistent with this, Clarkson et al. (2008) provide evidence, based on a sample of 191 firms in five high-pollution industries, that firms with better (worse) environmental performance on TRI emissions are more (less) likely to provide voluntary environmental disclosures in environmental and CSR reports.

Economic theory and empirical evidence suggest that firms that are more environmentally proactive, through initiatives such as implementing strong pollution-prevention programs and using renewable energy, have incentives to voluntarily disclose environmental information such as carbon emissions in order to reveal their environmental type, which is not directly observable by investors and other external stakeholders. Thus, we expect a positive coefficient on $STRNG_t$, our measure of

²⁵ We thank an anonymous reviewer for these insights.

environmentally proactive initiative, measured as the sum of KLD's environmentally proactive performance ratings for a firm in year t .

In contrast, economic theory offers competing predictions for environmentally damaging firms. On one hand, firms that are more environmentally damaging are less likely to voluntarily disclose environmental information if outsiders are unable to distinguish whether the non-disclosure is due to poor environmental performance or a desire to not reveal proprietary information (Verrecchia 1983). That is, environmentally damaging firms have incentives not to disclose in order to pool with other non-disclosure firms ascribed by outsiders as the "average" type (Verrecchia 1983; Healy and Palepu 2001). This reasoning argues for a negative coefficient for $CNCRN_t$, our measure of environmentally damaging action, where $CNCRN_t$ is the sum of KLD's environmentally damaging action ratings for a firm in year t . On the other hand, if outsiders are able to separate poor performers from good performers, then better performers within the pool of environmentally damaging firms have incentives to disclose. This is because absent such disclosure, uninformed investors and other external stakeholders will form more pessimistic beliefs about management's knowledge of the firms' environmental liabilities based upon publicly available information, thus increasing the probability of disclosure (see Li et al. 1997, Proposition 3d). This discussion predicts a positive $CNCRN_t$ coefficient. Given these two competing theories, we do not predict a sign for $CNCRN_t$.

We examine $STRNG_t$ and $CNCRN_t$ separately because KLD's proactive dimensions are largely distinct from the damaging dimensions. Moreover, prior research (Hughes, Anderson, and Golden 2001; Deegan 2002; Cho et al. 2012) suggests that the likelihood of environmental disclosures by environmentally proactive firms is dissimilar to that of environmentally damaging firms.

Other Independent Variables

$PROPDISCL_t$ measures the proportion of firms in an industry that disclose carbon emissions to the CDP. Consistent with Brown (2011), as more firms in a given industry disclose their carbon emissions, non-disclosers feel greater pressure to disclose them to avoid negative perceptions by outside stakeholders. In addition, we draw on theory showing that as more firms in an industry disclose, outsiders' assessments of the magnitude of non-disclosing firms' environmental liability increase, making it less costly for these firms to report higher liabilities (Li et al. 1997). Furthermore, managers of firms in the same industry face similar cost-benefit trade-offs and may contemporaneously come to a decision to disclose carbon emissions. Finally, based on conversations with sustainability consultants who have direct experience with the CDP questionnaire process, we assume that managers are able to exchange information on such voluntary disclosures with other firms in their industry. Thus, we capture both industry pressure and individually optimal disclosure decisions by including $PROPDISCL_t$, defined as the ratio of firms disclosing their carbon emissions in year t to the total firms in the industry in our sample in year t (using the two-digit SIC code). We expect a positive $PROPDISCL_t$ coefficient.

Prior research provides evidence of other systematic firm-level characteristics that may increase the likelihood that firms will respond to the CDP questionnaire and disclose their carbon emissions. For example, Stanny and Ely (2008) and Stanny (2013) examine characteristics of firms that respond to the CDP survey and find that size is positively correlated with the probability that firms will respond. Therefore, we include $SIZE_t$, measured using the log of the firm's total assets, and expect a positive sign on this variable. Firms that have a higher propensity to voluntarily disclose information in general may also be more likely to disclose their carbon emissions. Following Clarkson, Fang, Li, and Richardson (2010), we include the number of management forecasts issued by the firm during the year, denoted as MF_t , to control for the firm's general disclosure propensity. We expect a positive MF_t coefficient.

We control for firm growth by including the book-to-market ratio (BM_t) of the firm, but we do not predict a sign for BM_t . Prior research finds that firms with higher disclosure quality have lower cost of debt (Sengupta 1998). Consistent with higher-leverage firms providing higher-quality disclosures, we expect the coefficient on firm leverage, LEV_t , to be positive. Since the CDP is a consortium of large institutional investors, firms with higher institutional holdings may be more likely to disclose their carbon emissions, owing to investors' calls for more transparent disclosures about socially responsible activities (Plumlee, Brown, Hayes, and Marshall 2010). Alternatively, firms with fewer institutional investors may be more likely to respond to the CDP questionnaire to attract more institutional investors. Thus, we also control for the proportion of total shares outstanding held by institutional investors, II_t , from the Thomson Reuters 13-F database, but do not predict its sign.

Product market interactions affect firms' voluntary disclosures (Khanna, Palepu, and Srinivasan 2004) and European Union firms with higher proportions of international sales are more likely to disclose their carbon emissions (Stanny and Ely 2008). Therefore, to control for international product market interactions, we include in our logit model annual foreign sales as a proportion of total sales ($FRNSALE_t$) from the Worldscope database. S&P 500 firms that generate more earnings from outside the U.S. are more likely to respond to the CDP questionnaire, so we expect a positive $FRNSALE_t$ coefficient.

Consistent with Stanny (2013), we include an indicator variable, EPA_t , which is coded as 1 for firms that will be subject to the EPA's GHG mandatory reporting rule, and 0 otherwise.²⁶ Many of the firms in the $EPA = 1$ group also have industry-specific reporting requirements regarding their GHG emissions.²⁷ Therefore, we expect a positive coefficient for EPA_t . We include a lagged emissions indicator, $DISC\_CDP_{t-1}$, as an additional explanatory variable in our logit model. We expect a positive coefficient on $DISC\_CDP_{t-1}$, consistent with Stanny's (2013) finding that firms that responded to the CDP questionnaire in the previous year are eight times more likely to respond in the current year; that is, these disclosure decisions are "sticky." Finally, to control for industry-level characteristics, we include industry fixed effects at the two-digit SIC code level in Equation (1).

Firm-Value Effects of Decision to Disclose Carbon Emissions

As argued earlier, managers weigh the perceived costs and benefits of disclosing carbon emissions and choose to disclose only if the benefits of doing so outweigh the costs. Prior research on voluntary disclosures finds that *ceteris paribus*, firms that choose higher levels of voluntary disclosures have higher market values (Healy and Palepu 2001). Therefore, we also examine the firm-value effects of the decision to voluntarily disclose carbon emissions.

Using propensity score matching (Rosenbaum 2005), we compare the firm values for the firms that choose to disclose their carbon emissions with a matched sample of firms that choose to not disclose this information. To calculate the propensity scores we run a logit model using all the variables in Equations (1) and (2) above, except $TCO2_t$ and $DISC\_CDP_{t-1}$. We exclude $TCO2_t$ because it is available only for disclosing firms and $DISC\_CDP_{t-1}$ because it is the dependent variable in the previous year, and therefore not a "proper" covariate (Stuart and Rubin 2008). We match each disclosing firm to the closest non-disclosing firm(s) using two different matching algorithms, nearest-neighbor matching and caliper matching, and test for the difference in means and medians of firm value between the matched firms.

²⁶ Consistent with the EPA, we use the North American Industry Classification System (NAICS) to classify firms as $EPA = 1$ and $EPA = 0$.

²⁷ For instance, firms in the Oil and Gas industry are required to calculate their GHG emissions using techniques provided by the Compendium of GHG Emission Estimation Methodologies for the Oil and Gas Industry and the Petroleum Industry Guidelines for Reporting GHG Emissions.

There is a trade-off between: (1) including in the model all observable characteristics that distinguish the disclosing and non-disclosing firms; and (2) the ability to find a non-disclosing firm that matches each disclosing firm on all the included characteristics (Wooldridge 2012). Therefore, to increase the probability of finding a match, we also estimate the model after dropping some of the variables that are not significant in the original model. Propensity score matching alone is often sufficient to yield consistent or even efficient estimates. However, because it may not completely eliminate all systematic differences between the disclosing and non-disclosing firm means, we augment propensity score matching by using doubly robust regression (Imbens and Wooldridge 2007). Also, doubly robust regression allows us to obtain robust standard errors (Wooldridge 2012). More specifically, after matching our disclosing ($DISC\_CDP_t = 1$) and non-disclosing firms ($DISC\_CDP_t = 0$) using the propensity scores calculated with the logit model, we estimate Equation (1) for only the matched sample, replacing $TCO2_t$ with $DISC\_CDP_t$ and using the propensity scores as weights.

IV. RESULTS

Descriptive Statistics

Panel A of Table 2 shows summary statistics for our full sample of S&P 500 firms. Panel A also shows the sample breakdown by EPA group, those firms required by the EPA to disclose their carbon emissions for 2010 and thereafter ($EPA = 1$), and firms exempt from the EPA’s reporting rule ($EPA = 0$). Panel B of Table 2 provides summary statistics for our sample firms broken down into $DISC\_CDP = 1$, those that disclosed their carbon emissions publicly in the CDP questionnaire, and those that did not, $DISC\_CDP = 0$. We winsorize all the continuous variables at the 1 percent level on both tails of the distribution.

As shown in Table 2, Panel A, the distributions of both MKT and $TCO2$ are significantly skewed. Therefore, we report parametric tests for the means and nonparametric tests for the medians. Panel A confirms that our sample consists of extremely large firms, with mean (median) market value (MKT) of \$33.11 (\$16.01) billion. The mean of $TCO2$ is considerably larger than the median $TCO2$, indicating the presence of some large carbon emitters. Carbon emissions are significantly higher for the $EPA = 1$ group. Also, there is no significant difference in the market value of firms in the $EPA = 1$ and $EPA = 0$ groups.

Panel A of Table 2 also shows that the mean book values of total assets ($ASSET$) and total liabilities ($LIAB$) are significantly higher for the $EPA = 0$ group than for the $EPA = 1$ group. However, the respective median book values are not significantly different between the $EPA = 1$ and $EPA = 0$ groups at conventional levels. Taken together, the aforementioned firm characteristics indicate that the difference in carbon emissions between the two groups is not an artifact of firm size. Further, Panel B of Table 2 shows that the $DISC\_CDP = 1$ firms are bigger than the $DISC\_CDP = 0$ firms for MKT , $ASSET$, and $LIAB$, with significantly larger mean and median for the former group ($p = 0.00$).

Panel A of Table 2 shows that, consistent with institutional investors owning a significant portion of S&P 500 firms, both the mean and median institutional investors (II) ownership for our sample firms is about 80 percent. Panels A and B show that both the percentage of foreign sales, $FRNSALE$, and the percentage of firms disclosing emissions relative to the total firms in the industry, $PROPDISCL$, are higher for the $EPA = 1$ and $DISC\_CDP = 1$ groups. Interestingly, the mean and median levels of II are higher for firms in the $EPA = 0$ and $DISC\_CDP = 0$ groups.

Table 3 reports Spearman rank (Pearson) correlation coefficients above (below) the diagonal. Because a number of the variables used in our analyses are indicator variables and there is

TABLE 3
Correlation Coefficients

	MKT	TCO2	ASSET	LIAB	OPINC	CNCRN	STRNG	PROP- DISC	SIZE	MF	BM	LEV	II	FRN- SALE
MKT														
TCO2	0.267													
ASSET	0.642	0.403												
LIAB	0.539	0.426	0.965											
OPINC	0.818	0.366	0.748	0.693										
CNCRN	0.172	0.684	0.234	0.242	0.265									
STRNG	0.193	0.230	0.142	0.130	0.195	0.391								
PROPDISCL	0.102	0.474	0.146	0.155	0.172	0.401	0.346							
SIZE	0.642	0.403	1.000	0.965	0.748	0.234	0.142	0.146						
MF	<i>0.035</i>	<i>-0.025</i>	<i>-0.100</i>	<i>-0.093</i>	<i>0.035</i>	<i>0.035</i>	<i>0.119</i>	<i>0.113</i>	<i>-0.100</i>					
BM	-0.282	<i>0.103</i>	0.297	0.263	<i>-0.058</i>	<i>0.062</i>	-0.076	<i>0.028</i>	0.297	-0.145	<i>-0.159</i>	<i>0.046</i>	<i>0.027</i>	-0.249
LEV	-0.108	0.256	0.276	0.443	0.089	0.118	<i>-0.001</i>	0.116	0.276	<i>-0.029</i>	<i>0.046</i>	<i>-0.066</i>	<i>-0.054</i>	-0.226
II	-0.345	-0.277	-0.385	-0.361	-0.349	-0.217	-0.133	-0.180	-0.385	<i>0.044</i>	<i>0.007</i>	<i>-0.066</i>		<i>0.029</i>
FRNSALE	0.121	-0.178	-0.177	-0.226	<i>0.003</i>	0.115	0.355	0.204	-0.177	<i>0.038</i>	-0.288	-0.255	<i>0.033</i>	

Coefficients in italic are significant at $p < 0.10$.
Coefficients in bold are significant at $p < 0.01$.
Coefficients in plain text are not significant ($p > 0.10$).
 $n = 550$ for firm-value model and $n = 1,365$ for disclosure-choice model. See note a in Table 2.
Spearman rank (Pearson) correlation coefficients are below (above) the diagonal.
For variable definitions see Appendix A.

considerable skewness in the data, as described above, we discuss the Spearman rank correlation coefficients here. *TCO2* and *MKT* are positively correlated, which may be due to firm size; that is, larger firms with high *MKT* also have higher carbon emissions. The correlation between emissions (*TCO2*) and leverage (*LEV*) is positive and significant (0.256; $p < 0.01$), indicating that firms with higher emissions have more assets-in-place or are more capital-intensive (i.e., investments already made by the firm), compared to firms with lower emissions, which may have more growth opportunities (i.e., more real options for future investments) (Myers 1977). The correlation between *TCO2* and percentage of shares held by institutional investors (*II*) is -0.277 ($p < 0.01$), consistent with high-carbon-emitting firms having lower institutional investor holdings. Finally, the correlation between *TCO2* and the percentage of foreign sales (*FRNSALE*) is -0.178 ($p < 0.01$), indicating that higher- (lower-) carbon-emitting firms have a lower (higher) percentage of foreign sales.

Firm-Value Effects of Carbon Emissions

To correct for self-selection, we estimate the Heckman model (Heckman 1979; Maddala 1983) using the Full Information Maximum Likelihood (FIML) approach (Tucker 2010); that is, we estimate the firm-value model (Equation (1)) jointly with the disclosure-choice model (Equation (2)). Correcting for self-selection bias allows us to make inferences about the average effect of carbon emissions on firm value for all the firms in the sample, not just for the firms that disclose their emissions. Table 4 presents our results. We report t-statistics based on robust standard errors.

The coefficient on *TCO2_t* in Equation (1) for the full sample ($\beta_1 = -0.212$; $p < 0.01$) is negative and significant, consistent with H1. This result has economic significance because, on average, for every additional thousand metric tons of carbon emissions, firm value decreases by \$212,000. This translates to a \$1.4 billion reduction in firm-value moving from a firm in the first quartile of carbon emissions to one in the third quartile of emissions. The signs of coefficients on the other variables in the model are as expected.

To examine whether the markets value carbon emissions differently for the *EPA* = 1 and *EPA* = 0 groups, we estimate Equation (1) separately for each subgroup. Table 4 shows that the coefficient on *TCO2_t* for the *EPA* = 1 group is negative and significant ($\beta_1 = -0.182$; $p < 0.01$). Similarly, the coefficient on *TCO2_t* for the *EPA* = 0 group is negative ($\beta_1 = -0.176$); however, it is not significant at conventional levels ($p = 0.11$).

Although FIML is more efficient, it may be less robust than Limited Information Maximum Likelihood (LIML), the two-step estimator (Wooldridge 2002, 566). Therefore, we also run the analyses above using LIML (untabulated). Specifically, we calculate the Inverse Mills Ratio (IMR) from the disclosure-choice model, and then include it in the firm-value model.²⁸ We include industry-level fixed effects in the firm-value model. Our results are unchanged except for the *EPA* = 0 group, where the coefficient on *TCO2_t* is now significant at $p < 0.01$.

²⁸ Lennox, Francis, and Wang (2012) point out the importance of imposing “exclusion restrictions” when using the Heckman procedure. This is because absence of exclusion restrictions in the selection model (Equation (2)) can lead to biased coefficients that may be due to multicollinearity in the firm-value model (Equation (1)). The exclusion restriction requires at least one variable in the selection model that is conceptually excluded from the firm-value model. To satisfy the exclusion restriction, we include in our selection model a number of variables, such as *PROPDISCL*, *FRNSALE*, and *EPA*, which are related to the firm’s decision to disclose carbon emissions but do not directly affect firm value. Thus, we exclude these variables from the firm-value model. Lennox et al. (2012) point out the importance of testing for multicollinearity when using the Heckman procedure. We find that the variance inflation factor (VIF) for the IMR is less than 2 for the full sample and for the *EPA* = 1 and *EPA* = 0 subgroups, thus indicating that multicollinearity is not an issue.

TABLE 4
Firm-Value Effects of Carbon Emissions

Variable	Pred. Sign	Full Sample		EPA = 1		EPA = 0	
		Coeff.	Z-stat	Coeff.	Z-stat	Coeff.	Z-stat
TCO2 _{<i>t</i>}	H1: −	−0.212***	−3.72	−0.182***	−3.89	−0.176	−1.24
ASSET _{<i>t</i>}	+	1.129***	8.82	1.597***	5.95	1.228***	7.09
LIAB _{<i>t</i>}	−	−1.186***	−9.18	−1.624***	−5.97	−1.310***	−7.57
OPINC _{<i>t</i>}	+	4.421***	10.49	1.907**	2.06	5.200***	8.08
Disclosure-Choice Model (<i>DISC_CDP_t</i>)							
CNCRN _{<i>t</i>}	?	−0.034	−0.51	0.077	1.00	−0.279**	−2.14
STRNG _{<i>t</i>}	+	0.305***	4.29	0.283***	2.40	0.293***	2.65
PROPDISCL _{<i>t</i>}	+	0.025***	5.41	0.017**	2.08	0.030***	5.59
SIZE _{<i>t</i>}	+	0.394***	6.03	0.658***	5.46	0.331***	5.58
MF _{<i>t</i>}	+	0.014	1.26	0.005	0.30	0.016	1.14
BM _{<i>t</i>}	?	−0.619***	−4.23	−1.481***	−5.79	−0.446***	−2.95
LEV _{<i>t</i>}	+	−0.527§§§	−2.38	−0.840§§	−2.34	−0.444§§	−1.74
II _{<i>t</i>}	?	−0.001	−0.27	−0.008	−1.34	0.000	0.06
FRNSALE _{<i>t</i>}	+	0.009***	3.40	0.006	1.19	0.008**	2.26
DISC_CDP _{<i>t</i>−1}	+	1.286***	7.30	0.679***	3.24	1.607***	9.45
EPA _{<i>t</i>}	+	0.268	1.07				
Likelihood Ratio χ^2		2.88*		22.31***		0.14	
n		1,365		401		964	
Uncensored		550		229		321	

*, **, *** Denote significance at $p < 0.10$, < 0.05 , and < 0.01 , respectively, one-tailed where a directional prediction is made.

§§, §§§ Denote significance at $p < 0.05$ and < 0.01 , respectively, if the coefficient is significant in the opposite direction where a directional prediction is made. Otherwise, p-values are two-tailed.

For parsimony, coefficients on industry dummies are not reported.

Using the Full Information Maximum Likelihood (FIML) method, we estimate the Heckman (1979) model to correct for selection bias; that is, we estimate the firm-value model jointly with the disclosure choice model. We estimate the model on the full sample and separately for $EPA = 1$ and $EPA = 0$ firms. We include industry fixed effects at the two-digit SIC code level in both models. We report Z-statistics based on Huber-White robust standard errors:

$$MKT_t = \beta_0 + \beta_1 TCO2_t + \beta_2 ASSET_t + \beta_3 LIAB_t + \beta_4 OPINC_t + \varepsilon_t. \tag{1}$$

Disclosure-Choice Model:

$$DISC\_CDP_t = \beta_0 + \beta_1 CNCRN_t + \beta_2 STRNG_t + \beta_3 PROPDISCL_t + \beta_4 SIZE_t + \beta_5 MF_t + \beta_6 BM_t + \beta_7 LEV_t + \beta_8 II_t + \beta_9 FRNSALE_t + \beta_{10} DISC\_CDP_{t-1} + \beta_{11} EPA_t + \varepsilon_t. \tag{2}$$

$DISC\_CDP_t$ is an indicator variable equal to 1 if the firm publicly discloses carbon emissions, and 0 otherwise. The likelihood ratio χ^2 statistic is a test of the null hypothesis that there is no self-selection. The p-value for the goodness-of-fit test in this table is 0.00.

For variable definitions see Appendix A.

Also, Table 4 shows that the likelihood ratio Chi-square statistics that test the null hypothesis of no self-selection are significant for the full sample and for the $EPA = 1$ group, thus highlighting the importance of correcting for self-selection bias when examining the relationship between carbon emissions and firm value. Our results for the full sample as well as for the $EPA = 1$ group are both economically large and statistically significant. Overall, our results support our prediction that firm value is negatively associated with carbon emissions.

Self-Selection: Choice to Voluntarily Disclose Carbon Emissions

Table 4 also presents the results of estimating the disclosure-choice model in Equation (2). For the full sample and for firms in both the $EPA = 1$ and $EPA = 0$ groups, the coefficient on $STRNG_t$ is positive and significant ($p < 0.01$). These results indicate that in both groups, the higher the firms' scores on environmentally proactive initiatives, the more likely they are to disclose their carbon emissions. The coefficient on $CNCRN_t$ is not significantly different from zero for the full sample and for firms in the $EPA = 1$ group. In contrast, for firms in the $EPA = 0$ group, the coefficient on $CNCRN_t$ is negative and significant ($p < 0.05$), thus indicating that the higher the firms' scores on environmentally damaging actions, the less likely they are to disclose their carbon emissions.

The coefficient on $PROPDISCL_t$ is positive and significant for the full sample ($p < 0.01$) and for firms in both the $EPA = 1$ and $EPA = 0$ groups ($p < 0.05$ and $p < 0.01$, respectively), consistent with the probability of disclosure increasing as more firms in the industry disclose. The coefficient on $SIZE_t$ is positive and highly significant ($p < 0.01$), consistent with prior voluntary disclosure literature. Contrary to our expectations, firms with higher leverage, LEV_t , are less likely to disclose their carbon emissions. The coefficient on $FRNSALE_t$ is positive and significant for the full sample ($p < 0.01$) and for firms in the $EPA = 0$ group ($p < 0.05$). This is consistent with Stanny and Ely (2008), who find that the proportion of firms' foreign sales to total sales is positively associated with the firms' choice to respond to the CDP questionnaire. Finally, consistent with expectations, we find that firms that disclosed their carbon emissions in the prior year are significantly more likely to disclose their emissions in the current year ($p < 0.01$), indicating that the disclosure decision is sticky. The coefficients on the other variables in the model are either as predicted or not significantly different from 0.

Firm-Value Effects of Decision to Disclose Carbon Emissions

Our results indicate that the markets impose a penalty for firms' carbon emissions. This raises the question: "Why do firms voluntarily disclose their carbon emissions, given that the markets treat them as a cost or liability?" As discussed in Section II, it is not clear whether the markets will reward or penalize voluntary disclosers of carbon emissions. We address this question by using propensity score matching and doubly robust regression to examine the firm-value effects of the act of voluntarily disclosing carbon emissions, represented by $DISC\_CDP_t$.

Table 5, Panel A presents two logit models for the disclosure choice: (i) the full model with all the covariates from Equations (1) and (2); and (ii) a reduced model including only selected covariates. We use two matching algorithms: (i) nearest neighbor matching, and (ii) caliper matching with a distance of 0.01. To assess the quality of matching, we present the covariate balance for each model after matching. In the full model, we find that only some of the covariates are significant in explaining the firms' choice to disclose. The differences in means of the covariates for the matched firms indicate that the matched firms differ on only LEV_t and $OPINC_t$ ($p < 0.05$). Covariate balances for caliper matching are similar to the balances for nearest neighbor matching. To increase the number of firms that we are able to match, we next drop covariates that are both (i) statistically insignificant in the logit model and (ii) insignificantly different between the disclosing and non-disclosing firms after matching. In this reduced model, the means of the covariates are now different for MF_t and LEV_t ($p < 0.10$) and for $FRNSALE_t$ and EPA_t ($p < 0.05$) using nearest neighbor matching. None of the covariates are significantly different from 0 using caliper matching.

Table 5, Panel B presents the differences in the mean firm values for disclosing and non-disclosing firms. Using nearest neighbor matching, the difference in the mean firm values of the propensity score matched firms is approximately \$13.61 billion for the full model, and approximately \$11.42 billion for the reduced model, both significant at $p < 0.01$. Using caliper

TABLE 5
Firm-Value Effects of Decision to Disclose Carbon Emissions: Propensity-Score Matching
Panel A: Logit Model for Propensity Score Matching^a

DISC_CDP _{<i>t</i>}	Full Model				Reduced Model			
Variable	Estimates		Covariate Bal (Near Neighbor)		Estimates		Covariate Bal (Near Neighbor)	
	Coeff.	Z-stat	Discl.	Non-Discl.	Z-stat	Discl.	Non-Discl.	Z-stat
CNCRN _{<i>t</i>}	-0.036	-0.35	1.03	1.01		1.036	1.05	-0.13
STRNG _{<i>t</i>}	0.795***	6.84	0.97	0.88		0.979	0.94	0.64
PROPDISCL _{<i>t</i>}	0.061***	7.36	53.46	54.16		52.867	52.58	0.25
SIZE _{<i>t</i>}	0.547***	4.51	9.97	9.97		10.030	10.03	0.03
MF _{<i>t</i>}	0.034*	1.93	5.45	5.93		5.352	5.95	-1.71*
BM _{<i>t</i>}	0.030	0.71	0.47	0.51		0.379	0.42	-0.32
LEV _{<i>t</i>}	-0.243	-0.69	0.42	0.46		0.430	0.47	-1.77*
II _{<i>t</i>}	-0.003	-0.44	77.21	78.17		77.190	78.37	-1.42
FRNSALE _{<i>t</i>}	0.021***	4.77	32.15	33.73		32.277	36.05	-2.31**
EPA _{<i>t</i>}	0.729*	1.87	0.43	0.38		0.420	0.35	2.34**
ASSET _{<i>t</i>}	0.000	0.58	58,418	48,523		68,491	79,315	-1.010
LIAB _{<i>t</i>}	0.000	-0.65	46,112	38,952		56,218	69,450	-1.280
OPINC _{<i>t</i>}	0.000	0.86	3,841	2,685		4,208	4,073	0.310
n	1,234							
Treated	538							
Pseudo R ²	0.299							

* ** , *** Denote significance at p < 0.10, < 0.05, and < 0.01, respectively.

^a We match firms that disclose their carbon emissions, $DISC\_CDP_t = 1$ with firms that do not disclose their carbon emissions, $DISC\_CDP_t = 0$, using the following logit model that includes all the variables in Equations (1) and (2). We use two matching algorithms, nearest neighbor matching and caliper matching with a distance of 0.01. We include industry fixed effects at the two-digit SIC code level:

$$DISC\_CDP_t = \beta_0 + \beta_1 CNCRN_t + \beta_2 STRNG_t + \beta_3 PROPDISCL_t + \beta_4 SIZE_t + \beta_5 MF_t + \beta_6 BM_t + \beta_7 LEV_t + \beta_8 II_t + \beta_9 FRNSALE_t + \beta_{10} EPA_t + \beta_{11} ASSET_t + \beta_{12} LIAB_t + \beta_{13} OPINC_t + \varepsilon_t.$$

To increase the overlap between the disclosing and non-disclosing firms (i.e., the common support area), we also estimate a reduced model excluding covariates that were not significant in the logit regression, and covariates whose means did not differ significantly after matching. We report covariate balance means and Z-stats to test how equal (balanced) the disclosing and non-disclosing firms are for each covariate after matching only for nearest neighbor matching. Covariate balances are similar using caliper matching in the full model. In the reduced model there are no covariates that are significantly different between the disclosing and non-disclosing firms using caliper matching.

(continued on next page)

TABLE 5 (continued)

Panel B: Difference in Mean Firm Values of Propensity Score-Matched Firms^b

		Full Model				Reduced Model			
		Disclosers	Non-Disc.	Diff.	Z-stat	Disclosers	Non-Disc.	Diff.	Z-stat
Nearest Neighbor	Mean firm value	33,440.7	19,829.2	13,611.5***	4.50	34,077.0	22,661.5	11,415.5***	3.04
	n	538	696			559	727		
Caliper (0.01)	Mean firm value	29,615.2	19,718.8	9,896.4***	3.64	30,309.1	22,836.1	7,473.0**	2.26
	n	496	696			519	727		

*, **, *** Denote significance at $p < 0.10$, < 0.05 , and < 0.01 , respectively.

^b Using the propensity scores from the logit regressions in Panel A, we match our disclosing firms ($DISC\_CDP_t = 1$) with the non-disclosing firms ($DISC\_CDP_t = 0$). This panel provides the difference in means of the firm value for our matched firms, i.e., the average effect on the disclosing firms, using both nearest neighbor and caliper matching algorithms.

Panel C: Doubly Robust Regression Estimates of Mean and Median Firm Value^c

		Mean Firm Value ^d				Median Firm Value ^e			
		Full Model		Reduced Model		Full Model		Reduced Model	
		Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat
Nearest Neighbor	$DISC\_CDP_t$	6,352.7***	2.75	5,712.7**	2.55	2,869.4***	9.19	2,268.5***	8.34
	n	1,234		1,286		1,076		1,118	
Caliper (0.01)	$DISC\_CDP_t$	5,160.8***	3.53	5,062.5***	2.74	2,265.0***	4.79	2,097.8***	8.76
	n	992		1,038		992		1,038	

*, **, *** Denote significance at $p < 0.10$, < 0.05 , and < 0.01 , respectively.

^c After matching our disclosing and non-disclosing firms using the propensity scores calculated in Panel A for the two models, we examine the difference in the mean and median firm values between the disclosing and non-disclosing firms using the following regression:

$$MKT_t = \beta_0 + \beta_1 DISC\_CDP_t + \beta_2 ASSET_t + \beta_3 LIAB_t + \beta_4 OPINC_t + \varepsilon_t.$$

The regressions are run using propensity scores from the logit model in Panel A.

^d We include industry fixed effects in the regression model and report Z-statistics based on robust standard errors.

^e We do not include industry fixed effects in the median regressions because the model does not converge with industry fixed effects.

For variable definitions see Appendix A.

matching, the difference in the mean firm values is smaller: \$9.90 billion for the full model ($p < 0.01$) and \$7.47 ($p < 0.05$) billion for the reduced model.

To eliminate any remaining bias after propensity score matching, we run doubly robust regressions of firm value on the firms’ decision to disclose carbon emissions, $DISC\_CDP_t$. Results for the full model in Table 5, Panel C, show that the mean firm value of disclosing firms is significantly higher than that of non-disclosing firms by approximately \$6.35 billion (\$5.16 billion) for nearest neighbor (caliper) matching ($p < 0.01$). Similarly, results for the reduced model show that, on average, the mean firm value of disclosing firms is higher than non-disclosing firms by

approximately \$5.71 (\$5.06) billion for nearest neighbor (caliper) matching. These differences are significant at $p < 0.01$. The difference in mean firm value is smaller using doubly robust regression as compared to the results from propensity score matching only. This suggests that there was some residual bias, which we were able to remove using doubly robust regression.

We also estimate the effects of disclosing carbon emissions on median firm value, matching firms using propensity scores and then estimating quantile regressions using weights from the matching procedure (Table 5, Panel C). We do not include industry fixed effects in the quantile regression because our model does not converge, but we do include industry fixed effects in the matching procedure. Our results using nearest neighbor (caliper) matching for the reduced model show that the median firm value of disclosing firms is about \$2.27 (\$2.10) billion higher than that of non-disclosing firms. These differences are significant at $p < 0.01$. The differences in median firm values between disclosing and non-disclosing firms are slightly higher using the full model ($p < 0.01$).²⁹

Sensitivity Analyses

We conduct several sensitivity analyses to assess the robustness of our main results. First, we examine the relationship between changes in firm value, (ΔMKT_t) and changes in carbon emissions, $\Delta TCO2_t$ (Table 6). Consistent with our main results, we find that increases in carbon emissions are associated with decreases in firm value. In particular, the coefficient on $\Delta TCO2_t$ is negative and significant for the full sample ($p < 0.01$), for the $EPA = 1$ group ($p < 0.05$), and for the $EPA = 0$ group ($p < 0.10$).

Second, we scale both the dependent and independent variables in Equation (1) by the number of common shares outstanding. Our inferences (untabulated) regarding the negative association between $TCO2_t$ and MKT_t are unchanged for the full sample and for the $EPA = 1$ group. However, for the $EPA = 0$ group, the negative coefficient on $TCO2_t$ is now significant ($p < 0.01$). We also scale all of the variables by sales and our results are unchanged.

Third, we estimate the firm-value effects of carbon emissions using the Ohlson (1995) model, which provides a theoretical framework for the interpretation of the $TCO2$ coefficient in terms of the valuation implications of the firms' carbon emissions. Specifically, we estimate the following valuation model jointly with the disclosure-choice model specified in Equation (2):

$$MKT_t = \beta_0 + \beta_1 TCO2_t + \beta_2 CEQ_t + \beta_3 IBEI_t + \varepsilon_t, \quad (3)$$

where CEQ_t is the value of common equity of the firm and $IBEI_t$ is income before extraordinary items. We scale all the variables in the model by common shares outstanding. Our results (untabulated) show that the coefficient on $TCO2_t$ is negative and significant ($p < 0.05$ for the full sample and for the $EPA = 1$ group, and $p < 0.01$ for the $EPA = 0$ group). Other coefficients are also in the expected direction and significant ($p < 0.05$).

Fourth, we include firms with December fiscal year-end only. Because the carbon emissions data and market-value data are for the calendar year-end but the accounting data are for fiscal year-end, using firms with December fiscal year-end only better aligns the accounting data with carbon emissions and market-value data. Our results (untabulated) are consistent with the main results reported in Table 4. In other untabulated analyses, we also include return on assets as an additional control variable in the logit model. This variable is significant for the full sample and

²⁹ Our results do not imply that all firms are penalized equally for non-disclosure. Instead, the difference in market value that we document in Table 5 is the difference in the mean and median (Panel C) firm value for disclosing versus non-disclosing firms, conditional on various firm-level characteristics, including carbon emissions.

TABLE 6

Firm-Value Effects of Carbon Emissions

Changes Model

ΔMKT_t		Full Sample		$EPA = 1$		$EPA = 0$	
Variable	Pred. Sign	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
$\Delta TCO2$	H1: −	−0.045***	−2.45	−0.063**	−2.17	−0.020*	−1.28
$\Delta ASSET$	+	1.207***	4.90	2.158***	2.60	1.005***	4.52
$\Delta LIAB$	−	−0.040	−0.30	−0.942***	−2.58	0.074	0.76
$\Delta OPINC$	+	0.097**	1.73	0.028	0.21	0.121**	1.67
n		313		136		177	
R ²		0.2		0.2		0.33	

*, **, *** Denote significance at $p < 0.10$, < 0.05 , and < 0.01 , respectively, one-tailed where a directional prediction is made. Otherwise, p-values are two-tailed.

We estimate the following model to examine the percentage change in firm value as a function of the percentage change in carbon emission levels, controlling for other factors that are associated with firm value. We include firm fixed effects in all the estimations. We estimate the model on the full sample and separately for $EPA = 1$ and $EPA = 0$ firms. We report t-statistics based on Huber-White robust standard errors:

$$\Delta MKT_t = \beta_0 + \beta_1 \Delta TCO2_t + \beta_2 \Delta ASSET_t + \beta_3 \Delta LIAB_t + \beta_4 \Delta OPINC_t + \varepsilon_t.$$

Variable Definitions:

$\Delta MKT_t = [(MKT_t + DIV_t) - (MKT_{t-1} + DIV_{t-1})]/(MKT_{t-1} + DIV_{t-1})$;

DIV_t = dividend for year t ;

$\Delta TCO2_t = (TCO2_t - TCO2_{t-1})/TCO2_{t-1}$;

$\Delta ASSET_t = (ASSET_t - ASSET_{t-1})/ASSET_{t-1}$;

$\Delta LIAB_t = (LIAB_t - LIAB_{t-1})/LIAB_{t-1}$; and

$\Delta OPINC_t = (OPINC_t - OPINC_{t-1})/OPINC_{t-1}$.

The other variables are as defined in Appendix A.

for the $EPA = 0$ group ($p < 0.05$), and not significant for the $EPA = 1$ group. Our main results remain unchanged.

Finally, because our firm-value model controls only for assets and liabilities that are recognized on the firms’ balance sheets, we conduct sensitivity analyses (untabulated) to rule out the alternative explanation that the negative coefficient on $TCO2_t$ in Table 4 is proxying for other unbooked liabilities. We first add TRI emissions, TRI_t , in Equation (1) as a proxy for other unbooked environmental liabilities, following Clarkson et al. (2010).³⁰ Because TRI data are unavailable for some firms, we lose over 60 percent of the observations from the $EPA = 0$ group (we lose over 43 percent of the observations from the full sample). The coefficient on TRI_t is negative and significant for the full sample and for the $EPA = 1$ group, but it is not significant for the $EPA = 0$ group. We next include the firms’ credit ratings as a proxy for all other relevant information, including all unbooked liabilities. The coefficient on credit ratings is positive and significant ($p < 0.01$), consistent with the intuition that firms with higher credit ratings have higher firm values. For both these analyses, our results for $TCO2_t$ remain unchanged. Taken together, the results of our

³⁰ Specifically, we measure TRI_t as TRI emissions scaled by sales. Consistent with Clarkson et al. (2011), we then assign the scaled TRI emissions into deciles within each industry (two-digit SIC code).

sensitivity analyses are generally consistent with our main results and support our prediction that firm value is negatively associated with carbon emissions.³¹

V. CONCLUSION AND DISCUSSION

Using carbon emissions data for 2006 to 2008 that S&P 500 firms disclosed voluntarily, we find that the capital markets integrate both carbon emissions and the *act* of voluntary disclosure of this information into their firm valuations. The markets penalize all firms for their carbon emissions; firms that do not disclose their carbon emissions face a further penalty for non-disclosure. To our knowledge, this is the first study to provide empirical evidence on the firm-value effects of *both* the choice to voluntarily disclose carbon emissions and the magnitude of carbon emissions.

We predict and find a negative association between carbon emissions and firm value. Correcting for self-selection bias associated with managers' decisions to disclose carbon emissions, we find that for every additional thousand metric tons of carbon emissions, firm value decreases, on average, by \$212,000. Our finding of a negative association between carbon emissions and firm value begs the question: "If the capital markets penalize firms for their carbon emissions, then why do firms choose to disclose them?" We posit that managers weigh the costs and benefits of disclosing carbon emissions and choose to disclose only if the perceived benefits of doing so outweigh the perceived costs. Accordingly, we also estimate the firm-value effects of the act of voluntarily disclosing carbon emissions. Using propensity score matching and doubly robust regression, we find that the median firm value is about \$2.3 billion higher for firms that disclose their carbon emissions compared to firms that choose to not disclose them.

By providing scholarly evidence on the firm-value effects of carbon emissions, we supply managers with information that will help them make important decisions regarding the cost-benefit trade-offs of allocating resources to measuring, disclosing, and reducing carbon emissions. Our findings suggest that non-disclosure of carbon emissions may be costly for firms and is associated with a lower firm value, *ceteris paribus*. These findings are also important for boards of directors and managers because failure to integrate climate change into business strategy may reduce firm value (GS Sustain 2009).

Our findings are important to U.S. and international regulators and standard-setters as they work toward developing standards for measuring, assuring, and reporting on a firm's GHG statement. Our results suggest that capital markets incorporate information not only on the firm's choice to disclose its carbon emissions, but also on the level of these emissions. These valuations are consistent with demands by users and preparers for clearer guidelines from regulators and standard-setters for measuring and disclosing GHG emissions.

Of equal importance is the assurance of GHG emissions by an independent third party, which is currently not mandated in the U.S. Therefore, the firms in our sample choose whether to voluntarily disclose their emissions levels and, if they do so, then whether to provide assurance for their emissions and the type of assurance provider. Currently, many firms are required to have their carbon emissions assured, for example, by a public accounting firm, or verified by a governmental

³¹ We also address the conjecture that the insignificant results for the $EPA = 0$ group in Table 4 are due to a lack of power. This conjecture is corroborated by analyses that (1) reduce the number of observations without reducing noise; (2) reduce noise only; or (3) reduce the number of observations as well as noise. Regarding (1), when we include *TRI* in Equation (1), we lose over 60 percent of the observations from the $EPA = 0$ group. With this loss in power, the coefficient on *TCO2* is not statistically significant. Regarding (2), when we winsorize the data at the 5 percent level, the coefficient for the $EPA = 0$ group is significant at $p < 0.10$. Regarding (3), using only December fiscal year-end firms better aligns the accounting data and the emissions data. The *TCO2* coefficient for the $EPA = 0$ group is significant at $p < 0.10$. Thus, our tests support our conjecture that the statistically insignificant results for the $EPA = 0$ firms are due to a lack of power.

agency, such as the EPA through their Climate Leaders program. Furthermore, other firms have their emissions monitored by internal auditors. Also, some of the firms in our sample have voluntarily joined self-regulatory organizations, such as the Chicago Climate Exchange or the California Climate Action Registry, which require independent third-party assurance. This diverse institutional context points to the important role played by U.S. and international regulators and standard-setters in considering whether to require more uniformity in the assurance of GHG emissions with the goal of increasing the reliability of this information.

Having found evidence of a negative association between carbon emissions and firm value, in future research we plan to examine the association between carbon emissions and components of firm value. Another potentially fruitful area of research could address the challenges surrounding assurance of carbon emissions. Currently, the extent of independent third-party assurance and what constitutes assurance are ill-defined (Simnett et al. 2009b). International accounting research on assurance of GHG emission statements is gaining momentum (Zhou, Green, and Simnett 2012; Huggins, Green, and Simnett 2011), thus pointing to the increasing relevance of this issue. As more years of data become available from private and publicly available sources, future research could examine the value of assurance of carbon emissions and the association between assurance of carbon emissions and market value.

REFERENCES

- Akerlof, G. 1970. The market for “lemons”: Quality uncertainty and the market mechanism. *Quarterly Journal of Economics* 90:629–650.
- Barley, R. 2009. Drax in power struggle with S&P. *Wall Street Journal* (June 24).
- Barth, M. E., W. H. Beaver, and W. R. Landsman. 2001. The relevance of the value relevance literature for financial accounting standard-setting: Another view. *Journal of Accounting and Economics* 31: 77–104.
- Barth, M. E., and G. Clinch. 2009. Scale effects in capital markets-based accounting research. *Journal of Business Finance and Accounting* 36 (3/4): 253–288.
- Barth, M. E., and M. F. McNichols. 1994. Estimation and market valuation of environmental liabilities relating to Superfund sites. *Journal of Accounting Research* 32 (Supplement): 177–209.
- Blacconiere, W. G., and D. M. Patten. 1994. Environmental disclosures, regulatory costs, and changes in firm value. *Journal of Accounting and Economics* 18 (3): 355–377.
- Brown, J. L. 2011. The spread of aggressive corporate tax reporting: A detailed examination of the corporate-owned life insurance shelter. *The Accounting Review* 86 (1): 23–57.
- Campbell, K., S. E. Sefcik, and N. S. Soderstrom. 1998. Site uncertainty, allocation uncertainty, and Superfund liability valuation. *Journal of Accounting and Public Policy* 17: 331–366.
- Campbell, K., S. E. Sefcik, and N. S. Soderstrom. 2003. Disclosure of private information and reduction of uncertainty: Environmental liabilities in the chemical industry. *Review of Quantitative Finance and Accounting* 21: 349–378.
- Castelo Branco, M., and L. Lima Rodrigues. 2006. Corporate social responsibility and resource-based perspectives. *Journal of Business Ethics* 69: 111–132.
- Chapple, L., P. M. Clarkson, and D. L. Gold. 2013. The cost of carbon: Capital market effects of the proposed emission trading scheme (ETS). *Abacus* 49 (1): 1–33.
- Cho, C. H., M. Freedman, and D. M. Patten. 2012. Corporate disclosure of environmental capital expenditures. *Accounting, Auditing, and Accountability Journal* 25 (3): 486–507.
- Clarkson, P. M. 2012. The valuation relevance of environmental performance: Evidence from the academic literature. In *Contemporary Issues in Sustainability Accounting, Assurance and Reporting*, Chapter 2, edited by S. Jones, and J. Ratnatunga, 11–42. Bingley, U.K.: Emerald Group Publishing Limited.
- Clarkson, P. M., X. Fang, Y. Li, and G. Richardson. 2010. *The Relevance of Environmental Disclosures: Are Such Disclosures Incrementally Informative?* Working paper, University of Queensland, Georgia State University, and University of Toronto.

- Clarkson, P. M., Y. Li, and G. D. Richardson. 2004. The market valuation of environmental expenditures by pulp and paper companies. *The Accounting Review* 79 (2): 329–353.
- Clarkson, P. M., Y. Li, G. D. Richardson, and F. P. Vasvari. 2008. Revisiting the relation between environmental performance and environmental disclosure: An empirical analysis. *Accounting, Organizations and Society* 33: 303–327.
- Clarkson, P. M., Y. Li, G. D. Richardson, and F. P. Vasvari. 2011. Does it really pay to be green? Determinants and consequences of proactive environmental strategies. *Journal of Accounting and Public Policy* 30: 122–144.
- Connors, E., H. H. Johnston, and L. Silva-Gao. 2013. The informational value of Toxics Release Inventory performance. *Sustainability, Accounting, Management and Policy Journal* 4 (1): 32–55.
- Cormier, D., and M. Magnan. 1997. Investors' assessment of implicit environmental liabilities: An empirical investigation. *Journal of Accounting and Public Policy* 16: 215–241.
- Deegan, C. 2002. The legitimizing effect of social and environmental disclosures: A theoretical foundation. *Accounting, Auditing, and Accountability Journal* 15: 282–311.
- Dhaliwal, D., O. Z. Li, A. Tsang, and Y. G. Yang. 2011. Voluntary nonfinancial disclosure and the cost of equity capital: The initiation of corporate social responsibility reporting. *The Accounting Review* 86 (1): 59–100.
- Eccles, R. G., M. P. Krzus, and G. Serafeim. 2011. Market interest in nonfinancial information. *Journal of Applied Corporate Finance* 23 (4): 113–127.
- Environmental Protection Agency. 2009. *Mandatory Reporting of Greenhouse Gases*. 74 FR 56260. Available at: <http://www.gpo.gov/fdsys/pkg/FR-2009-10-30/pdf/E9-23315.pdf>
- Epstein, M. J. 2008. *Making Sustainability Work—Best Practices in Managing and Measuring Corporate, Social, Environmental, and Economic Impacts*. San Francisco, CA: Berrett Koehler.
- Ernst & Young. 2011. Climate change and sustainability: Shareholders press boards on social and environmental risks. *Ernst & Young LLP* 1–5.
- Fornaro, J. M., K. A. Winkelman, and D. Glodstein. 2009. Accounting for emissions. *Journal of Accountancy* (July): 40–45.
- GS Sustain. 2009. *Change is Coming: A Framework for Climate Change—A Defining Issue of the 21st Century*. New York, NY: The Goldman Sachs Group, Inc.
- Hart, S. L. 1995. A natural-resource-based view of the firm. *The Academy of Management Review* 20 (4): 986–1014.
- Healy, P. M., and K. G. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31: 405–440.
- Heckman, J. J. 1979. Sample selection bias as a specification error. *Econometrica* 47: 153–162.
- Holthausen, R. W., and R. L. Watts. 2001. The relevance of the value-relevance literature for financial accounting standard-setting. *Journal of Accounting and Economics* 31: 3–75.
- Huggins, A., W. J. Green, and R. Simnett. 2011. The competitive market for assurance engagements on greenhouse gas statements: Is there a role for assurers from the accounting profession? *Current Issues in Auditing* 5 (2): A1–A12.
- Hughes, S. B., A. Anderson, and S. Golden. 2001. Corporate environmental disclosures: Are they useful in determining environmental performance? *Journal of Accounting and Public Policy* 20: 217–240.
- Hughes II, K. E. 2000. The value relevance of nonfinancial measures of air pollution in the electric utility industry. *The Accounting Review* 75 (2): 209–228.
- IGEL/Knowledge@Wharton. 2012. *Special Report. Greening the Supply Chain: Best Practices and Future Trends*. Initiative for Global and Environmental Leadership and Knowledge@Wharton, June, 1–28. Available at: <http://knowledge.wharton.upenn.edu/special-report/greening-the-supply-chain-best-practices-and-future-trends>
- Imbens, G., and J. Wooldridge. 2007. *What is New in Econometrics: Estimation of Average Treatment Effects Under Unconfoundedness. Lecture 1, Summer*. Cambridge, MA: National Bureau of Economic Research.

- Institute of Chartered Accountants in England and Wales. 2004. *Information for Better Markets—Sustainability: The Role of Accountants*. London, U.K.: ICAEW.
- International Auditing and Assurance Standards Board (IAASB). 2012. *Assurance Engagements on Greenhouse Gas Statements*. International Standard on Assurance Engagements No. 3410. New York, NY: International Federation of Accountants.
- IRRCi/Trucost. 2009. *Carbon Risks and Opportunities in the S&P 500*. Investor Responsibility Research Center Institute and Trucost Plc. Available at: http://www.irrcinstitute.org/pdf/irrc_trucost_0906.pdf
- Johnston, D. M., S. E. Sefcik, and N. S. Soderstrom. 2008. The value relevance of greenhouse gas emissions allowances: An exploratory study in the related United States SO₂ market. *European Accounting Review* 17: 747–764.
- Johnston, J. S. 2005. *Signaling Social Responsibility: on the Law and Economics of Market Incentives for Corporate Environmental Performance*. Scholarship At Penn Law. Working paper 66, University of Pennsylvania Law School.
- Khanna, T., K. G. Palepu, and S. Srinivasan. 2004. Disclosure practices of foreign companies interacting with U.S. markets. *Journal of Accounting Research* 42: 475–508.
- Lennox, C. S., J. R. Francis, and Z. Wang. 2012. Selection models in accounting research. *The Accounting Review* 87 (2): 589–616.
- Li, Y., G. B. Richardson, and D. B. Thornton. 1997. Corporate disclosure of environmental liability information: Theory and evidence. *Contemporary Accounting Research* (Fall): 435–474.
- Maddala, G. S. 1983. *Limited-Dependent and Qualitative Variables in Econometrics*. New York, NY: Cambridge University Press.
- Milgrom, P. R. 1981. Good news and bad news: Representation theorems and applications. *Bell Journal of Economics* (Autumn): 380–391.
- Moser, D. V., and P. R. Martin. 2012. A broader perspective on corporate social responsibility research in accounting. *The Accounting Review* 87 (3): 797–806.
- Myers, S. 1977. The determinants of corporate borrowings. *Journal of Financial Economics* 5: 147–175.
- Ohlson, J. A. 1995. Earnings, book values, and dividends in equity valuation. *Contemporary Accounting Research* (Spring): 661–687.
- Plumlee, M., D. Brown, R. Hayes, and S. Marshall. 2010. *Voluntary Environmental Disclosure Quality and Firm Value: Further Evidence*. Working paper, The University of Utah and Portland State University.
- PricewaterhouseCoopers. 2012. *Do Investors Care About Sustainability? Seven Trends Provide Clues*. (March). New York, NY: PwC.
- Rosenbaum, P. R. 2005. Observational study. In *Encyclopedia of Statistics in Behavioral Science*, Vol. 3, edited by B. S. Everitt, and D. C. Howell, 1451–1462. Chichester, PA: John Wiley & Sons, Ltd.
- Sengupta, P. 1998. Corporate disclosure quality and cost of debt. *The Accounting Review* 73 (4): 459–474.
- Sharfman, M. P., and C. S. Fernando. 2008. Environmental risk management and the cost of capital. *Strategic Management Journal* 29: 569–592.
- Simnett, R., M. Nugent, and A. L. Huggins. 2009a. Developing an international assurance standard on greenhouse gas statements. *Accounting Horizons* 23 (4): 347–363.
- Simnett, R., A. Vanstraelen, and W. F. Chua. 2009b. Assurance on sustainability reports: An international comparison. *The Accounting Review* 84 (3): 937–967.
- Stanny, E. 2013. Voluntary disclosures of emissions by US firms. *Business Strategy and the Environment* 22 (3): 145–158.
- Stanny, E., and K. Ely. 2008. Corporate environmental disclosures about the effects of climate change. *Corporate Social Responsibility and Environmental Management* 15: 338–348.
- Stuart, E. A., and D. B. Rubin. 2008. Best practices in quasi-experimental designs: Matching methods for causal inference. In *Best Practices in Quantitative Methods*, edited by J. Osborne, 155–176. Thousand Oaks, CA: Sage Publications.
- Thaler, R. H., and C. R. Sunstein. 2009. *Saving the planet. In Nudge: Improving Decisions About Health, Wealth, and Happiness*, 185–198. London, U.K.: Penguin Books.

Tucker, J. W. 2010. Selection bias and econometric remedies in accounting and finance research. *Journal of Accounting Literature* 29: 31–57.

Vagus, S. 2012. Microsoft to adopt internal cap-and-trade practice for carbon emissions. *Hydrogen Fuel News* (May 14). Available at: <http://www.hydrogenfuelnews.com/microsoft-to-adopt-internal-cap-and-trade-practice-for-carbon-emissions/853650>

Verrecchia, R. 1983. Discretionary disclosure. *Journal of Accounting and Economics* 5: 179–194.

Walley, N., and B. Whitehead. 1994. It’s not easy being green. *Harvard Business Review* (May-June): 46–52.

Wooldridge, J. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: The MIT Press.

Wooldridge, J. 2012. Treatment effect estimation with unconfounded assignment. *American Accounting Association/Financial Accounting and Reporting Section Workshop*, Chicago. Available at: http://www.ruf.rice.edu/~kr10/slides_fars_2012.pdf

Zhou, S., W. Green, and R. Simnett. 2012. *The Decision to Assure and Assurance Provider Choice: Evidence From the GHG Assurance Market*. Working paper, Australian School of Business.

Ziliak, S. T., and D. N. McCloskey. 2004. Significance redux. *The Journal of Socio-Economics* 33: 665–675.

APPENDIX A

Variable Definitions

<i>MKT</i>	=	market value of common equity at the end of the calendar year, calculated as: SHROUT * PRC (in \$ millions), where (SHROUT * PRC) = common shares outstanding multiplied by price per share;
<i>TCO2</i>	=	total carbon emissions in metric tons for year <i>t</i> (in thousands);
<i>ASSET</i>	=	book value of the firm’s total assets (AT) at the end of fiscal year <i>t</i> (in \$ millions);
<i>LIAB</i>	=	book value of the firm’s total liabilities (LT) at the end of fiscal year <i>t</i> (in \$ millions);
<i>OPINC</i>	=	firm’s operating income (in \$ millions) after depreciation (OIADP) for fiscal year <i>t</i> ;
<i>CNCRN</i>	=	number of damaging ratings (concerns) for the firm identified in KLD;
<i>STRNG</i>	=	number of proactive ratings (strengths) for the firm identified in KLD;
<i>PROPDISCL</i>	=	ratio of the number of firms in the two-digit SIC industry code with publicly available carbon emissions to the total number of firms in the industry in our sample;
<i>SIZE</i>	=	log of the firm’s total assets at the end of the fiscal year;
<i>MF</i>	=	number of management forecasts issued by the firm during the year;
<i>BM</i>	=	firm’s book-to-market ratio;
<i>LEV</i>	=	firm’s leverage, measured as (DLTT + DLC)/(DLTT + DLC + CEQ);
<i>II</i>	=	percentage of total shares outstanding held by institutional investors, from the Thomson Reuters 13-F database;
<i>FRNSALE</i>	=	firm’s foreign sales as a percentage of total sales for the year, from the Worldscope database;
<i>EPA</i>	=	an indicator variable equal to 1 if the firm operates in an industry that will be required by the EPA’s GHG Mandatory Reporting Rule to report its GHG emissions, and 0 otherwise; and
<i>DISC_CDP</i>	=	an indicator variable equal to 1 if the firm discloses its carbon emissions to the CDP and to the public, and 0 otherwise.

Narrative Disclosure and Earnings Performance: Evidence from R&D Disclosures

Kenneth J. Merkley
Cornell University

ABSTRACT: This paper examines how earnings performance relates to firms' narrative R&D disclosure decisions. The unique nature of R&D investments and financial statements' limited ability to communicate the value of such investments highlight the role of narrative disclosure as a supplement to the financial statements. I predict and find that current earnings performance (adjusted for R&D expense) is negatively related to the quantity of narrative R&D disclosure. Conducting a content analysis of the detail, tone, and readability of narrative R&D disclosures, I find that managers adjust R&D disclosures based on earnings performance to provide relevant information rather than to obfuscate performance. Finally, I provide evidence that market participants find narrative R&D disclosure informative because it significantly affects sell-side analyst behavior, disclosure information content, and information asymmetry.

Keywords: *narrative disclosure; research and development; earnings; content analysis.*

Data Availability: *Data are available from sources identified in the text.*

I. INTRODUCTION

Narrative disclosure provides a channel for managers to convey contextual information about their firms to market participants. This type of disclosure can bridge the gap between a firm's financial statement numbers and its underlying business fundamentals. For narrative disclosure to perform this function, managers must be willing to modify their disclosures based on

For their helpful insights and suggestions, I thank the members of my dissertation committee: Reuven Leavy (Co-Chair), Greg Miller (Co-Chair), Venky Nagar, Tyler Shumway, and Jeff Smith. I also thank Bill Baber, Beth Blankespoor, Sanjeev Bhojraj, Jim Cannon, Craig Chapman, Ted Christensen, Julia D'Souza, Jeff Hoopes, Raffi Indjejikian, Feng Li, Russell Lundholm, Andrea Merkley, Mark Nelson, Shiva Rajgopal, Darren Roulstone, Cathy Shakespeare, Hal White, Chris Williams, Mohan Venkatachalam, and workshop participants at Cornell University, Emory University, Georgetown University, The Ohio State University, The Pennsylvania State University, Northwestern University, Southern Methodist University, University of California, Berkeley, University of Michigan, and Washington University in St. Louis for valuable comments and suggestions. I thank Amy P. Hutton (editor), John Harry Evans III (senior editor), and two anonymous referees for their helpful comments.

This paper is based on my dissertation titled "More than Numbers: R&D-Related Disclosure and Firm Performance." Supplemental material can be accessed by clicking the link in Appendix A.

Editor's note: Accepted by Amy P. Hutton.

Submitted: December 2011
Accepted: October 2013
Published Online: November 2013

their firms' changing financial performance and investors' changing information needs. This study examines whether managers adjust their narrative disclosures based on current earnings performance and whether such disclosures are informative to market participants.

I focus on how firms' earnings performance relates to their narrative disclosures of research and development (R&D) activities. Although not necessarily generalizable to other settings, R&D highlights the value of narrative disclosure and is important because companies invest billions of dollars each year in R&D projects to create future value and growth (Wolfe 2010). Nevertheless, accounting standards mandate expensing R&D rather than capitalizing these investments, and they limit R&D disclosure on the face of the financial statements. Furthermore, R&D is idiosyncratic in nature with high levels of risk and information asymmetry, and standard accounting measures do not easily communicate its performance.

These distinctive features imply that financial statements likely do not communicate effectively the value of R&D, and this inadequate communication gives rise to information problems (Lev 1999; Franzen, Rodgers, and Simin 2007; Morricone and Oriani 2009). These problems might encourage managers to provide detailed R&D disclosures to help investors assess the nature and causes of variation in earnings performance, which would improve the firms' information environments (Healy and Palepu 2001; Glassman 2003; Joos and Plesko 2005). Yet, it is unclear whether managers will make effective, credible R&D disclosures and whether investors will, in turn, view these disclosures as informative given the high complexity and uncertainty of R&D activities.

In this paper, I conduct three separate analyses to address whether managers adjust R&D disclosure in response to the information demands of investors based on changing earnings performance. First, I examine how R&D disclosure quantity varies in relation to earnings performance. Second, I conduct a detailed content analysis to examine whether R&D disclosure detail also varies in relation to earnings performance to assess whether changes in R&D disclosure quantity provide relevant information or whether they are consistent with other disclosure motivations such as obfuscation. Finally, I assess whether analysts and investors find narrative R&D disclosure informative by examining whether R&D disclosure quantity relates to measures of analyst forecast behavior, information content, and information asymmetry.

I measure the quantity of narrative R&D disclosures as the number of R&D-related sentences in the 10-K filings of firms that invested in R&D from 1996–2007. I categorize a sentence as related to R&D if it contains specific R&D keywords or phrases. I find that firms reporting lower (higher) earnings performance adjusted for R&D expense provide more (less) R&D disclosure. This result persists after controlling for disclosure-related factors such as information environment, type of operations, uncertainty, financing-related incentives, and firm fixed effects. This result is also robust to the use of a changes specification. In cross-sectional tests, I find that earnings performance relates more negatively to R&D disclosure quantity when measures of investor information demand are higher, suggesting that firms respond to investor information needs. I find no evidence that the negative relation between R&D disclosure quantity and earnings performance relates to measures of proprietary cost.

I also find that in contrast to the R&D disclosure quantity results, earnings performance relates positively to a more general measure of the quantity of narrative earnings-related information. This finding is consistent with prior literature on earnings-based disclosures and provides evidence that a more general earnings-disclosure relation does not drive the relation between earnings performance and the quantity of narrative R&D disclosure. These results demonstrate that earnings can influence distinct types of narrative disclosure differently.

Although these results suggest that firms adjust R&D disclosure quantity to provide more relevant information to investors, firms might also adjust R&D disclosure without necessarily providing more relevant information. Managers might provide more (less) vague R&D disclosures when performance is lower (higher), reflecting a changing level of disclosure “fluff.” To examine this possibility, I test whether earnings performance relates to measures of R&D disclosure detail,

including the amount of numerical content, forward-looking statements, and repetition. I find that as earnings performance declines, R&D disclosures contain more forward-looking statements and marginally more repetition. I find no evidence that the amount of numerical content varies in relation to earnings. Overall, these findings suggest that firms modify both the quantity and the details of their R&D disclosure based on earnings performance.

It is also possible that managers adjust narrative R&D disclosure opportunistically to obfuscate earnings performance or to distract investors from key issues. To consider this possibility, I examine whether earnings performance relates to the overall tone and readability of narrative R&D disclosure in a manner consistent with opportunistic behavior. I find that narrative R&D disclosure tone relates positively to earnings performance, suggesting that firms do not use narrative R&D disclosure to put a positive spin on poor earnings performance. Moreover, after controlling for the number of R&D-related sentences, I find no evidence that earnings performance relates to the readability of R&D disclosures, which is inconsistent with firms attempting to obfuscate earnings performance.

Finally, I provide evidence that market participants find narrative R&D disclosures informative. I find that narrative R&D disclosure quantity is positively related to analyst following and analyst forecast accuracy and negatively related to analyst forecast dispersion. I also find that R&D disclosure quantity is positively related to disclosure information content and negatively related to information asymmetry around the 10-K filing event window. Together, these results suggest that market participants find R&D disclosure informative, not merely disclosure “fluff,” and that managers do not use it opportunistically, on average. Overall, the results are consistent with economic theories that suggest that informative disclosures can benefit firms by mitigating problems relating to information asymmetry and adverse selection, which should ultimately benefit firms in terms of higher equity valuations and lower costs of capital (Healy and Palepu 2001).

My findings contribute to prior literature in several ways. First, the results have implications for research on firms’ R&D activities, an area of economic importance that is the focus of numerous studies.¹ Although prior literature highlights the relative scarcity of required R&D disclosure, our understanding of discretionary R&D disclosure is limited to the results of a few studies that focus on specific industries and years (Entwistle 1999; Lev 2001; Jones 2007). I provide the first large-scale study of public firms’ disclosure of R&D information and the first to consider textual properties such as R&D disclosure tone and readability. My findings suggest that firms modify R&D disclosures based on earnings performance to provide relevant information to market participants, which in turn reduces information asymmetry. These results are particularly relevant to future debates on R&D disclosure choices as firms and regulators respond to the changing economy that focuses on intangible activities such as R&D.

Although one must be cautious when generalizing these results to non-R&D disclosure settings, my paper adds to our general understanding of narrative and qualitative disclosure by providing original evidence that the effect of earnings performance on narrative disclosure depends on the disclosure type. Prior literature generally finds that earnings performance is positively related to the frequency of earnings-related disclosures and overall disclosure quality, suggesting that this result holds for disclosure in general (e.g., Lang and Lundholm 1993; Miller 2002).² My results suggest that disclosure decisions are more complex and depend not only on performance, but also

¹ Examples from economics, finance, and accounting include Griliches (1981, 1998), Jaffe (1986), Hall (1993), Sougiannis (1994), Lev and Sougiannis (1996), Lev and Zarowin (1999), Aboody and Lev (2000), Boone and Raman (2001), Chan, Lakonishok, and Sougiannis (2001), and Kothari, Laguerre, and Leone (2002).

² Exceptions are recent studies that find that the information content of verbal disclosures in conference calls is higher when performance is poor (Frankel, Mayew, and Sun 2010; Matsumoto, Maarten, and Roelofsen 2011). My findings complement these studies by providing evidence that the relation between performance and disclosure content depends on the type of disclosure as well as evidence that narrative R&D and narrative earnings disclosures are different.

on the subject matter to be disclosed. In particular, I find that earnings performance influences narrative R&D disclosure decisions differently from more general narrative earnings-related disclosures along several dimensions, including the quantity of numerical and forward-looking content and the extent of disclosure repetition.

Next, Section II reviews prior literature and the hypotheses; Section III then describes the sample and empirical measures. Section IV presents the results, and Section V concludes.

II. PRIOR LITERATURE AND HYPOTHESES

The R&D Setting

A large body of literature examines R&D funding, performance, and valuation. As reviewed by Lev (2001) and Grandi, Hall, and Oriani (2009), these studies find that R&D affects market performance, expected profits, and future cash flows. Yet R&D is also highly firm-specific and is associated with higher information asymmetry and uncertainty (Aboody and Lev 2000; Boone and Raman 2001; Chan et al. 2001; Kothari et al. 2002). Furthermore, few mandatory R&D disclosure requirements exist, and accounting standards require the immediate expensing of most R&D activities, which likely understates their long-run value (Lev 2001; Hand and Lev 2003; Morricone and Oriani 2009).³ Together, these issues create the potential for significant information problems between managers and investors.

Notwithstanding the importance of R&D information, our understanding of how firms communicate it in financial reports is limited. Narrative R&D disclosures account for a significant amount of R&D information but are difficult for researchers to capture and not available in large commercial databases. Although generally limited to small samples of select industries and short time periods, the few prior studies offer helpful insights into the R&D disclosure process.

Entwistle (1999) examines the R&D disclosure environment of 113 technology firms listed on the Toronto Stock Exchange in 1994. His field interviews suggest that managers consider both capital and product market concerns in their R&D disclosure decisions. He finds that the quantity of R&D-related disclosure varies by industry and by the amount of R&D spending. Similarly, Jones (2007) examines the quantity of R&D information provided by 119 U.S. firms from four R&D-related industries in 1998. She finds that R&D disclosure is associated with lower analyst forecast errors for both earnings and sales. Guo, Lev, and Zhou (2004) examine the product-related disclosures in the IPO prospectuses of 49 biotech companies from 1995 to 1997. They find that the stage of product development, the availability of patent protection, and the presence of venture capital backing influence biotech firms' disclosures. They also find that the quantity of this disclosure is negatively associated with measures of information asymmetry.⁴

Although these studies provide insights about the determinants and consequences of R&D disclosure quantity, they do not specifically examine the role of earnings performance and do not consider other disclosure characteristics such as tone and readability. I provide original evidence on the properties of narrative R&D disclosures and compare these properties with earnings-related

³ This generalization can depend on the presence of particular R&D arrangements. Current regulations require that firms disclose (1) the total R&D expenses for each income statement period presented (SFAS 2), (2) the amount of any in-process R&D acquired through acquisitions (SFAS 141 and 142), and (3) the terms of any R&D arrangements accounted for as a contract to perform R&D for others (SFAS 68).

⁴ In addition, two working papers examine firms' disclosure of R&D-related information outside of the firms' formal financial reporting. James and Shaver (2009) examine the disclosures of 302 firms in the communications and pharmaceutical industries. Their evidence suggests that firms with stronger strategic positions are more likely to provide press releases regarding R&D accomplishments. Nichols (2009) examines the disclosure of product-related and business expansion announcements. Neither paper specifically examines the role of earnings performance.

disclosures. In addition, I extend prior work by using a larger sample of firms that spans many different industries and a longer time period, allowing me to base my tests on within-firm variation by using regression models with firm fixed effects.

The Relation between R&D Narrative Disclosure and Current Earnings

Variation in a firm's reported earnings performance influences investors' demands for disclosure information and managers' willingness to supply it (Bagnoli and Watts 2007). Although prior studies generally document a *positive* relation between earnings performance and the frequency of earnings-related disclosures (Miller 2002; Chen, Matsumoto, and Rajgopal 2011; Houston, Lev, and Tucker 2010), this finding will not necessarily hold for narrative R&D disclosure given the unique characteristics of R&D investments and their effect on investors' information demands.

I predict that the quantity of a firm's narrative R&D disclosure relates *negatively* to current earnings performance. As earnings performance declines, managers' incentives to provide R&D disclosure increase because investors demand more information about a firm's activities. Lower earnings performance produces a noisier signal of firm value because earnings losses and decreases typically do not persist (Hayn 1995; Koch and Sun 2004). In addition, lower earnings performance is associated with higher information asymmetry (Ertimur 2004; Brown, Lo, and Hillegeist 2009; Rogers, Skinner, and Van Buskirk 2009). These issues are particularly salient for firms with R&D investments because the nature of these activities and their required accounting treatment make interpreting earnings performance more difficult (Joos and Plesko 2005; Franzen et al. 2007). Therefore, investor demand for R&D information should be higher when earnings are lower, indicating a negative relation between the quantity of narrative R&D disclosure and current earnings performance.⁵

H1: Narrative R&D disclosure quantity is negatively related to current earnings performance.

H1 suggests that earnings performance relates to the quantity of narrative R&D disclosure because of investors' demand for R&D information. If this is true, then we should expect the strength of this relation to vary based on the overall level of investor information demand. For example, influential market participants such as financial analysts or institutional investors could intensify the influence of earnings performance on firms' narrative R&D disclosure decisions. Thus, I predict that the negative relation between R&D disclosure and current earnings performance hypothesized in H1 increases when a firm's overall level of information demand increases.

H2: The relation between narrative R&D disclosure quantity and current earnings performance is more negative when investor information demand is higher.

Alternative Explanations

Although the previous hypotheses rely on demand-side arguments, there are other possible reasons for a negative relation between the quantity of a firm's narrative R&D disclosures and earnings performance. Managers might adjust narrative R&D disclosure based on earnings performance to mitigate proprietary costs, to distract investors with vague disclosures, or to obfuscate performance information.

⁵ This argument will not necessarily hold for other types of narrative disclosure. Compared with general narrative disclosures, narrative R&D disclosures are more future-oriented, more difficult to quantify, and more closely related to firm strategy. To address this point, I compare the R&D disclosure results to those using more general narrative earnings disclosures in the empirical analysis.

I first consider the role of proprietary costs. Earnings performance indicates to some degree the success of prior R&D investments. Detailed information on those investments potentially benefits product market competitors (Dontoh 1989; Segerstrom 1991; Sadka 2004). In particular, prior studies suggest that earnings performance is positively related to concerns about proprietary costs (Verrecchia 1983; Harris 1998; Leuz 2004; Botosan and Stanford 2005), implying that firms will provide less R&D disclosure when earnings performance is better to limit competitors' ability to adjust important aspects of their operations (Lippman and Rumelt 1982; Barney 1991). Thus, if proprietary costs are associated with earnings performance, then we would expect a negative relation between narrative R&D disclosure and current earnings performance. If this is the case, we should also expect the negative relation hypothesized in H1 to increase when a firm's overall level of proprietary costs increases. Accordingly, I make the following hypothesis:

H3: The relation between narrative R&D disclosure quantity and current earnings performance is more negative when proprietary costs are higher.

Managers also have *opportunistic* incentives to provide R&D information. For example, to divert investors' attention from poor earnings performance, managers might provide more R&D disclosure, and they might provide less disclosure when reporting higher earnings makes distraction unnecessary. Similarly, managers might adjust R&D disclosure to rationalize cuts in R&D spending or changes in R&D activities that are driven by lower earnings performance. For example, Lansford (2006) finds that firms strategically time their patent disclosures to coincide with poor earnings reports.

To test whether my findings are consistent with these possibilities, I examine how earnings performance relates to the nature and detail of a firm's R&D disclosures. Under these alternative explanations, firms would provide more (less) R&D disclosure when earnings are lower (higher) but would not necessarily adjust disclosure detail. For example, under the opportunistic explanation, firms would likely provide more vague or "fluff" disclosures and even lower the level of actual disclosure detail. Managers might also adjust the tone of R&D disclosures to spin earnings news or to obfuscate earnings performance, as shown in Li (2008). In contrast, under the investor demand explanation, firms would provide more R&D disclosure detail to provide relevant information to investors and would not use tone to color the implications of earnings news. I examine these issues empirically in Section IV.

Market Consequences of Narrative R&D Disclosure

If managers provide R&D disclosure not based on opportunistic motivations but to help investors understand a firm's operations, then we should expect R&D disclosure to influence investors' views of a firm. However, because R&D information is often complex and difficult to process, whether this information would benefit all investors is unclear. For example, R&D disclosure is likely to benefit more sophisticated investors at the expense of others, consistent with Miller's (2010) conclusion that small investors find it too costly to process information in complex 10-K disclosures. Prior studies suggest that financial analysts represent a relatively sophisticated group and have the ability to evaluate R&D information in a timely manner (Schipper 1991; Barth, Kasznik, and McNichols 2001; Jones 2007; Kimbrough 2007). If analysts view R&D disclosure as useful, then greater amounts of narrative R&D disclosure should be associated with higher analyst following, better earnings forecast accuracy, and lower forecast dispersion.⁶

⁶ In general, analyst following changes very little from period to period. This lack of variation could limit the ability of my empirical tests to find statistically significant results.

H4: Narrative R&D disclosure quantity relates positively to analyst coverage and earnings forecast accuracy and negatively to analyst forecast dispersion.

Similarly, if R&D disclosure provides useful information to investors, then we should expect such disclosure to influence their response to financial reports. Nevertheless, it is difficult to determine whether narrative R&D disclosure provides good or bad news because such analysis requires a measure of investor expectations (Guo et al. 2004). Thus, rather than making directional predictions about stock price movements, I consider whether the release of narrative R&D disclosure relates to the overall information content of financial reports. In addition, if narrative R&D disclosure helps to bridge the information gap between the firm and external stakeholders, then we should expect such disclosure to be associated with lower information asymmetry.

H5: Narrative R&D disclosure quantity relates positively to disclosure information content and negatively to information asymmetry.

III. DATA AND VARIABLE MEASUREMENT

My sample includes firm-year observations from 1996 to 2007 and requires that data be available from the Compustat, CRSP, and SEC EDGAR databases. In addition, I require that all observations have at least \$1 million in assets and report non-zero R&D expense. My main sample includes 22,482 firm-year observations.

Measuring Narrative R&D Disclosure Quantity

I measure narrative R&D disclosure quantity by using a computerized content analysis of 10-K filings. This method has several advantages. First, it allows sampling from a broad range of industries and firms across a relatively long time period. Second, this measure allows examination of narrative disclosures provided concurrently with the financial statements. This focus reduces selection bias because all firms with material R&D investments must provide a minimum level of disclosure in the 10-K filing. Third, prior research and the author's private conversations with competitive intelligence professionals suggest that 10-K filings are an important source of information (Previts, Bricker, Robinson, and Young 1994; Rogers and Grant 1997; Leder 2003; Brown and Tucker 2010; Lehavy, Merkley, and Li 2011).

To measure narrative R&D disclosure, I develop a dictionary of common R&D keywords and phrases through careful examination of a random selection of 150 10-K filings.⁷ To ensure that my dictionary is reasonable, I consulted industry personnel on R&D-related disclosure topics and compared my list to those of Entwistle (1999) and James and Shaver (2009). I use "regular expression" routines from the Perl programming language to search each 10-K and identify narrative R&D disclosures at the sentence level. Because it is difficult to construct disclosure measures based on subjective assessments of specific information items, I measure narrative R&D disclosure ($R\&D\ DISC_{QTY}$) as the total number of R&D-related sentences in firms' 10-K filings. For comparison purposes, I also create other narrative disclosure measures based on the total number of earnings-related sentences ($EARN\ DISC_{QTY}$) and the total number of non-R&D sentences ($NonR\&D\ DISC_{QTY}$).

Although sentence count measures are simple in construction, they represent a relatively crude disclosure proxy and will not likely capture certain important nuances in narrative disclosure. For example, firms can change the quality of their disclosure without necessarily changing the number

⁷ An appendix containing the complete word list and examples of different R&D disclosure topics is available, see Appendix A.

of sentences. In addition, the measure assumes that all sentences are weighted equally and that disclosure quantity accurately captures the amount of disclosure information. I partially address some of these limitations in Section IV through a content analysis of disclosure detail, tone, and readability. Nonetheless, these measures are limited in scope because they also focus on sentence-level data, and I cannot be sure that they capture the true underlying message of firms' disclosures.

Other Variable Definitions

Earnings Performance

I measure current earnings performance by using adjusted return-on-assets (*adjROA*) defined as annual operating earnings before R&D and advertising expense scaled by ending total assets, similar to Sougiannis (1994). Significant evidence exists regarding the role of earnings as an important performance measure to investors. Earnings are also important to managers either directly as a component of their compensation contract or indirectly in relation to stock prices. In addition, earnings are a visible performance measure and receive considerable attention from investors and the financial press. Consistent with this view, Graham, Harvey, and Rajgopal (2005, 13) report that CFOs surveyed believe that earnings are "the most important financial metric to external constituents."

Control Variables

The regression analysis includes controls for various factors that relate both to disclosure and earnings performance, including firms' information and disclosure environments, investment mix, information uncertainty, and financing incentives. Prior research suggests that a firm's information and disclosure environment is a function of its size, the influence of outside monitors, such as analysts and institutional investors, and its maturity (Lang and Lundholm 1993; Healy and Palepu 2001). I employ various proxies to capture these variables. I measure *SIZE* as the logarithm of the market value of equity, *ANALYSTS* as the number of analysts following the firm at the beginning of the fiscal period, and *%INST* as the percentage of institutional ownership at the beginning of the fiscal period. I control for the overall quality of firms' disclosures by using the number of management forecasts issued during the reporting period (Nagar, Nanda, and Wysocki 2003; Lehavy et al. 2011) and certain characteristics of non-R&D disclosures in the 10-K, including quantity, detail, tone, or readability, depending on the variable of interest. This method helps to address concerns that general disclosure trends drive my results. In addition, I control for the firm's life cycle or maturity by using the number of years since the firm first appeared on the CRSP database (*AGE*) and its square ($AGE \times AGE$) to account for potential nonlinearity.

Prior research suggests that investment mix influences the importance of capital and product market considerations to a firm (Kothari et al. 2002). Entwistle (1999) finds that firms with higher R&D expenditures provide higher R&D-related disclosure. I measure the extent of R&D investment as the ratio of R&D expense to total operating expense (*R&D/OPX*). I include the square of this variable ($R\&D/OPX \times R\&D/OPX$) to control for nonlinearity. I also measure the extent of intangible investment by using the book-to-market ratio (*BM*), which might additionally relate to aspects of proprietary cost. I measure capital intensity (*CAPINT*) as the ratio of tangible assets (PP&E and inventories) to total assets. Firms with more tangible investments might have less incentive to provide R&D-related disclosures because they have higher collateral in the form of physical assets whose value is less uncertain (Kothari et al. 2002).

Prior literature provides mixed evidence regarding the effect of information uncertainty on disclosure (Lang and Lundholm 1993). Although investors prefer more disclosure when uncertainty is high, managers might be less able to provide accurate information and might have higher disclosure costs. I use the standard deviation of monthly stock returns over the reporting period

(*RETVOL*) and the standard deviation of *adjROA* over the prior three years to control for prior information uncertainty. These measures capture information uncertainty relating to both general performance and to the uncertainty surrounding the payoffs to R&D investments.

Because of the uncertain nature of the returns to R&D investments, firms that invest in R&D rely on equity financing more than debt. Differences between debt and equity interests might create incentives for different R&D-related disclosure information. Prior research suggests that firms might alter their disclosure behavior around capital offerings (Lang and Lundholm 1993). Therefore, I include controls for leverage (*LEVERAGE*) measured as the ratio of debt to total assets and stock issuance (*STOCK_ISS*) measured as an indicator variable that equals 1 if the firm has a positive net equity issuance in the current year.

In addition to these controls, I also include firm and year fixed effects in many of the analyses. Firm fixed effects help to control for unobserved time-invariant factors that are not accounted for in other control variables. These controls are particularly important because firms' disclosure policies and firm performance might be related to other unobservable characteristics that might confound my inferences.

IV. RESULTS

Summary Statistics

Table 1 presents the descriptive statistics for my sample. Consistent with prior research, the distribution of the size of firms that invest in R&D measured by total sales, total assets, and market value is right skewed with means (medians) of \$1,300 (\$94) million, \$1,400 (\$124) million, and \$3,200 (\$197) million, respectively. The average (median) firm in my sample has been public for 15 (9) years, is followed by 4.4 (2) analysts, and institutions own approximately 38 (34) percent of its outstanding shares. These averages are smaller than those of more general samples because firms that invest in R&D tend to be smaller, less profitable firms. The average (median) adjusted return-on-assets (*adjROA*) is 0.06 (0.11). The average (median) ratio of R&D expense to total operating expense is 0.17 (0.10). The average (median) firm has a book-to-market ratio of 0.50 (0.37) and a mean (median) debt-to-asset ratio (*LEVERAGE*) of 0.17 (0.09).

Table 2 provides descriptive statistics for the measure of narrative R&D disclosure quantity. As shown in Panel A, the average firm in my sample discloses 30.87 R&D-related sentences. This number is likely higher than what might be anticipated if firms only disclosed the minimum required R&D expense amounts. As a benchmark, I also examine the number of earnings-related disclosures. The average number of R&D-related sentences is lower than that of earnings-related disclosures (48.32 sentences). However, this number still represents a significant quantity of narrative R&D disclosure, especially given critics' contention that firms provide this information only for compliance purposes (Morricone and Oriani 2009). I also find that the average amount of narrative R&D disclosure increases across time (untabulated).

Table 2, Panel B ranks the Fama-French 48 classification of industries by narrative R&D disclosure quantity. Consistent with prior studies (Entwistle 1999), pharmaceutical firms provide the greatest quantity of R&D disclosure, averaging approximately 83 R&D-sentences per 10-K. Firms in industries that create high-tech/computer products (Medical Equipment, Business Services, Electronic Equipment, Measuring and Control Equipment, and Computers) provide approximately 24 to 30 R&D-related sentences on average. The remaining industries produce somewhat less R&D disclosure, averaging between 11 to 18 sentences.

Table 2, Panel C provides statistics on the percentage of firms' R&D disclosures that address each of 15 specific R&D topics. I assign one or more topics to an R&D-related sentence if it or the sentences directly following it contain words relating to a particular R&D topic. The topics are not

TABLE 1
Sample Descriptive Statistics

Variable	Mean	Median	Std. Dev.
<i>adjROA</i>	0.06	0.11	0.24
<i>Sales</i> (\$mil)	1,300.00	94.00	4,200.00
<i>Total Assets</i> (\$mil)	1,400.00	124.00	4,700.00
<i>Market Value</i> (\$mil)	3,200.00	197.00	18,000.00
<i>AGE</i>	15.00	9.00	16.00
<i>ANALYSTS</i>	4.40	2.00	6.03
<i>%INST</i>	0.38	0.34	0.32
<i>MFCOUNT</i>	1.49	0.00	2.72
<i>R&D/OPX</i>	0.17	0.10	0.21
<i>BM</i>	0.50	0.37	0.52
<i>CAPINT</i>	0.29	0.27	0.20
<i>RETVOL</i>	0.19	0.15	0.13
<i>adjROAVOL</i>	0.09	0.05	0.12
<i>LEVERAGE</i>	0.17	0.09	0.23
<i>STOCK_ISS</i>	0.55	1.00	0.50

This table provides descriptive statistics for the main sample of 22,482 firm-year observations.

Variable Definitions:

adjROA = operating income before R&D and advertising expense scaled by ending total assets;
AGE = number of years that a firm has been on the CRSP database;
ANALYSTS = number of analysts following the firm at the beginning of the fiscal period;
%INST = percentage of institutional ownership at the beginning of the fiscal period;
MFCOUNT = number of management forecasts issued during the fiscal period;
R&D/OPX = ratio of R&D expense to total operating expense;
BM = ratio of book value to market value at the end of the fiscal period;
CAPINT = ratio of PP&E and inventories to total assets;
RETVOL = standard deviation of monthly returns during the fiscal period;
adjROAVOL = standard deviation of *adjROA* over the past three years;
LEVERAGE = ratio of total debt to total assets; and
STOCK_ISS = an indicator variable coded to 1 if the firm has a positive net stock issuance during the fiscal period, and 0 otherwise.

mutually exclusive because a single R&D disclosure can address multiple areas. For example, an R&D-related sentence from a pharmaceutical firm might discuss the progress of current projects in its drug pipeline as well as the potential competition. On average, the most common R&D disclosures involve prior R&D expenditures (14 percent), competition (12 percent), facilities (12 percent), and strategy (11 percent). Firms generally provide less information about topics such as R&D progress (5 percent), employees (3 percent), and funding (2 percent). Overall, these data suggest that firms provide important information to help alleviate information asymmetry concerns, but the data are qualified by firms’ desire not to disclose information such as R&D progress that might be valuable to competitors.

Earnings Performance and the Quantity of Narrative R&D Disclosure

H1 considers whether narrative R&D disclosure quantity is negatively related to current earnings performance. To test this hypothesis, I first examine univariate correlations. Table 3, Panel A presents Spearman and Pearson correlations of earnings (*adjROA*) with total narrative R&D disclosure, total narrative earnings-related disclosures, and with the respective number of R&D-

TABLE 2
Descriptive Information on Narrative R&D Disclosure Quantity

Panel A: Narrative Disclosure Quantity (n = 22,482)

Variable	Mean	Median	Std. Dev.
10-K R&D Disclosure	30.87	20	34.65
10-K Earnings Disclosure	48.32	40	31.47

Panel B: Narrative R&D Disclosure Quantity by Industry

Industry	n	Mean	Median	Std. Dev.
Pharmaceutical Products	3,216	83.35	70.00	55.93
Medical Equipment	1,780	29.99	26.00	19.25
Business Services	3,693	27.82	24.00	23.38
Electronic Equipment	2,894	26.80	24.00	16.76
Measuring and Control Equipment	1,176	26.09	22.00	16.67
Computers	2,009	24.05	22.00	14.76
Chemicals	643	18.43	13.00	20.66
Electrical Equipment	446	16.66	10.00	17.51
Machinery	1,366	15.87	11.00	12.65
Other	4,766	13.96	10.00	13.70
Auto	493	11.85	9.00	11.05

Panel C: Subject of Narrative R&D Disclosures

Variable	Mean	Median	Std. Dev.
%R&D to Prior Periods	0.14	0.09	0.15
%R&D Competition	0.12	0.10	0.11
%R&D Facilities	0.12	0.09	0.12
%R&D Strategy	0.11	0.09	0.11
%R&D to Operations	0.07	0.04	0.08
%R&D to Sales	0.06	0.00	0.10
%R&D Collaboration	0.05	0.00	0.10
%In-Process R&D	0.04	0.00	0.11
%Patents	0.05	0.01	0.07
%R&D Progress	0.05	0.00	0.09
%R&D Employees	0.03	0.00	0.06
%R&D Funding	0.02	0.00	0.04
%R&D Tax Credit	0.02	0.00	0.04
%Regulation	0.01	0.00	0.02
%R&D Market	0.00	0.00	0.01

This table provides descriptive information about the narrative R&D disclosure quantity measure that is calculated as the number of 10-K sentences that refer to R&D-related information. Panel A provides statistics on the overall narrative R&D disclosure measure and benchmarks it against a comparable score for earnings-related narrative disclosures. Panel B provides descriptive information about narrative R&D disclosure for selected industries. Industries are based on the Fama-French 48 classification. Panel C provides information regarding the average percentage of firms’ R&D-related sentences that address specific topics.

TABLE 3
Narrative R&D Disclosure Quantity and Earnings Performance

Panel A: Correlation of Earnings Performance and Narrative R&D Disclosure

Variable	Pearson	Spearman
Overall Disclosures		
Total 10-K R&D Disclosures	−0.1212***	−0.1235***
10-K Earnings Disclosures	0.2877***	0.3395***
R&D Disclosure Categories		
R&D to Prior Periods	−0.0869***	−0.1001***
R&D Competition	−0.0974***	−0.1012***
R&D Facilities	−0.0550***	−0.0630***
R&D Strategy	−0.0824***	−0.0981***
R&D to Operations	−0.0460***	−0.0536***
R&D to Sales	0.1478***	0.2031***
R&D Collaboration	−0.0579***	−0.1572***
In-Process R&D	0.0700***	0.0856***
Patents	−0.1677***	−0.1974***
R&D Progress	−0.1113***	−0.1593***
R&D Employees	−0.0552***	−0.0752***
R&D Funding	−0.1004***	−0.1652***
R&D Tax Credit	−0.0170***	−0.0516***
Regulation	−0.0927***	−0.1335***
R&D Market	−0.0280***	−0.0343***

*** Indicates significance at the 0.01 level or lower.

Panel B: Multivariate Regression Results with Firm Fixed Effects

	Variable	<i>Ln(R&D DISC_{QTY})</i>	<i>Ln(EARN DISC_{QTY})</i>
	Intercept	0.203 [0.88]	−0.370*** [−3.12]
Earnings Performance	<i>adjROA</i>	−0.048** [−1.99]	0.195*** [9.61]
Information Environment	<i>SIZE</i>	0.061*** [8.92]	0.032*** [5.95]
	<i>ANALYSTS</i>	0.002 [1.23]	0.002 [1.41]
	<i>%INST</i>	−0.011 [−0.33]	0.073*** [2.91]
	<i>MFCOUNT</i>	−0.002 [−0.72]	0.010*** [6.67]
	<i>Ln(NonR&D DISC_{QTY})</i>	0.289*** [23.69]	0.496*** [48.63]
	<i>AGE</i>	−0.007 [−0.31]	0.026*** [2.88]
	<i>AGE × AGE</i>	0.000*** [4.53]	−0.000*** [−2.99]

(continued on next page)

TABLE 3 (continued)

	Variable	$Ln(R\&D\ DISC_{QTY})$	$Ln(EARN\ DISC_{QTY})$
Investment Type	$R\&D/OPX$	1.695*** [12.31]	−0.351*** [−3.07]
	$R\&D/OPX \times R\&D/OPX$	−1.286*** [−9.42]	0.195* [1.74]
	BM	0.072*** [7.06]	0.061*** [7.52]
	$CAPINT$	0.004 [0.07]	−0.145*** [−3.56]
	Uncertainty	$RETVOL$ 0.039 [1.47]	−0.059** [−2.37]
Financing	$adjROAVOL$	0.051 [1.32]	−0.037 [−0.99]
	$LEVERAGE$	0.073*** [3.19]	−0.008 [−0.32]
	$STOCK\_ISS$	0.021*** [2.72]	0.000 [0.03]
Observations		22,482	22,482
Adjusted R ²		89.6%	84.4%

*, **, *** $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.

This table examines the relation between narrative R&D disclosure quantity and current earnings performance. Panel A provides Pearson and Spearman correlations for the association of *adjROA* (operating income before R&D and advertising expense scaled by ending total assets) with the total number of R&D disclosure sentences as well as the total sentences for each R&D topic area. Panel B provides multivariate results based on the regression of the natural log of R&D disclosure quantity on *adjROA* and additional controls. *SIZE* is defined as the natural log of market value at the end of the fiscal year. *NonR&D DISC_{QTY}* is number of 10-K sentences not related to R&D. The regressions include firm and year fixed effects. t-statistics use standard errors that are robust to heteroscedasticity and clustered by firm. All other variables are defined as in Table 1.

related disclosures on specific topics. I find that consistent with H1, earnings performance correlates negatively with total narrative R&D disclosure quantity. Conversely, I find a positive relation between earnings and 10-K earnings-related disclosure, a topic that has different characteristics from R&D-related information and is better recognized in the financial statements. In addition, earnings performance relates negatively to almost all of the individual R&D topic disclosure measures. On average, these correlations range from approximately −0.02 to −0.20, with exceptions that include the quantity of discussions about “R&D to Sales” and “In-Process R&D.”⁸ In particular, earnings performance (*adjROA*) relates more negatively to firm-specific issues such as R&D progress, patents, funding, and employees.

Next, I provide further evidence by estimating the following multivariate regression that includes controls for various disclosure determinants examined in prior literature:

⁸ One possible reason for the positive correlation for “R&D to Sales” is that firms with better earnings might have incentives to tout R&D spending amounts because poor-performing firms might be more prone to cut R&D spending (Bushee 1998). The positive association for “In-Process R&D” likely occurs because better-performing firms are more likely to participate in acquisitions.

$$\begin{aligned}
Ln(R\&D\ DISC_{QTY}) = & \beta_0 + \beta_1 adjROA + \beta_2 SIZE + \beta_3 ANALYSTS + \beta_4 \%INST \\
& + \beta_5 MFCOUNT + \beta_6 Ln(NonR\&D\ DISC_{QTY}) + \beta_7 AGE \\
& + \beta_8 AGE \times AGE + \beta_9 R\&D/OPX + \beta_{10} R\&D/OPX \times R\&D/OPX \\
& + \beta_{11} BM + \beta_{12} CAPINT + \beta_{13} RETVOL + \beta_{14} adjROAVOL \\
& + \beta_{15} LEVERAGE + \beta_{16} STOCK\_ISS + \text{firm and time fixed effects} + \varepsilon.
\end{aligned}
\tag{1}$$

The dependent variable is the natural logarithm of $R\&D\ DISC_{QTY}$.⁹ Table 3, Panel B reports the results of the estimation using OLS regression with t-statistics in brackets based on standard errors that are robust to heteroscedasticity and clustered by firm.

Consistent with H1, the results in the first column of Table 3, Panel B, show that the coefficient on $adjROA$ is -0.048 ($p < 0.05$), suggesting that firms with lower (higher) earnings performance provide more (less) narrative R&D disclosure.¹⁰ I find that consistent with prior literature, R&D disclosure is positively related to size and to the amount of disclosure in other areas as measured by $NonR\&D\ DISC_{QTY}$. I also find that firms with higher investment in innovation as measured by $R\&D/OPX$, higher leverage, and positive net stock issuances provide more narrative R&D disclosure.

In addition, I find that $adjROA$ is *positively* related ($p < 0.001$) to the quantity of general narrative earnings-related disclosures (second column of Table 3, Panel B) rather than *negatively* as in the case of R&D disclosure. Although not generalizable to firms without R&D investments, these results provide evidence that current earnings performance does not relate to all narrative disclosures in the same way, an issue that the disclosure literature has not sufficiently addressed.

Although the negative relation between R&D disclosure and earnings performance is consistent with the investor demand explanation, the results might also be attributable to endogeneity issues. For example, when firms have lower earnings expectations, they might cut R&D investments and then provide more R&D disclosure to explain the unexpected cut. Alternatively, managers might provide less R&D disclosure and more non-R&D disclosure to divert investors' attention from R&D cuts. Consistent with this last point, the coefficients on $R\&D/OPX$ in Table 3, Panel B suggest that firms provide less (more) narrative R&D disclosure and more (less) narrative earnings disclosure as R&D expenditures decrease (increase). To further address the potential impact of changes in earnings expectations as an omitted variable, I replicate my results by using a smaller sample with available management forecast data to measure earnings expectations during the year. Untabulated results indicate that my inferences in Table 3, Panel B are robust to controlling for earnings expectations.¹¹ Nevertheless, my results might still be attributable to endogeneity issues and other alternative explanations, some of which I address in the subsequent analyses.

The Role of Information Demand and Proprietary Costs

Although the previous results are consistent with the information demand motivation of H1, they might also relate to proprietary cost concerns. In particular, H2 and H3 suggest that both

⁹ Because $R\&D_{DISC}$ has a skewness of 2.8 in my sample, I use the logarithmic transformation of $R\&D_{DISC}$ to help alleviate this issue. The results are the same in terms of direction and statistical significance when using the untransformed variable. The intercept included in the model represents the base intercept for the combination of the first firm and year in the sample and has little interpretive value. I follow this convention throughout the paper.

¹⁰ Using a changes specification based on year-to-year changes in the variables, I also find evidence of a negative and statistically significant relation ($p < 0.05$) between earnings and narrative R&D disclosure.

¹¹ I compute the forecast news by subtracting the most recent consensus analyst earnings forecast and scale by price prior to the consensus date. I then use the forecast news from the first forecast for each fiscal year and the average forecast news as controls for changing earnings expectations.

information demand and proprietary costs issues can influence the negative relation between narrative R&D disclosure quantity and current earnings performance. Because it is difficult to measure investor information demand and proprietary costs directly in my setting, I use empirical proxies from prior studies to create interaction variables to test these hypotheses.

Information Demand

Table 4, Panel A provides the results based on adding terms for the interaction of earnings and proxies for investor information demand to the model in Equation (1). I use the number of analysts and the percentage of institutional ownership as proxies because these parties are known to influence firms’ information environments (Drake, Roulstone, and Thornock 2012). I also use an indicator variable for years after the passage of Regulation Fair Disclosure (Reg FD) because it limited private communication between managers and analysts, increasing the demand for public disclosure. I use the level of earnings guidance activity measured by the number of earnings forecasts issued during the year under the assumption that investors expect more R&D disclosure when guidance activity is higher. Finally, I use an indicator variable for whether a firm had poor stock return performance measured as a return less than zero because information demand is likely to be greater (lower) when other performance measures are lower (higher).

To evaluate H2, I consider the coefficient estimates on the interaction term, *adjROA* × *Information Demand*, in Table 4, Panel A to test whether the negative relation between R&D disclosure and earnings is more negative when information demands are higher. Consistent with H3, the interaction term is negative and statistically significant ($p < 0.05$) for the analyst, Reg FD, earnings forecast, and poor stock return proxies. The interaction term is negative but not statistically significant when using institutional ownership as a proxy. Overall, these results are consistent with the information demand motivation for H1.

Proprietary Costs

Table 4, Panel B provides the results of using proxies for proprietary costs. I follow Bamber and Cheon (1998), Karuna (2007), and Li (2010b) to measure proprietary costs based on firms’ product substitutability measured by the sales-weighted average of the gross margin multiplied by -1 , ease of entry by competitors measured by the natural logarithm of the sales-weighted average PP&E of the industry multiplied by -1 , industry competition in terms of the industry four-firm concentration ratio multiplied by -1 ,¹² and growth measured by the market-to-book ratio. I define these variables so that higher values indicate higher proprietary costs.

Because measuring proprietary costs is difficult, I evaluate the validity of these measures by first including them separately in the regression model. I find in untabulated results that consistent with proprietary costs explanations, R&D disclosure is negatively related to each variable and statistically significant ($p < 0.05$) for all but the measure of product substitutability. I next test H3 by interacting these measures with earnings performance. As shown in Panel B, the coefficients on all the interaction terms are insignificant. Thus, although I find support for the role of proprietary costs in general, the evidence does not support the proprietary cost motivation for H1. These tests, however, are not necessarily conclusive because of the complex nature of proprietary costs and the measurement issues surrounding them.

¹² Data for the four-firm concentration ratios are available from the U.S. economic census (<http://www.census.gov/econ/concentration.html>), and other data are obtained from Compustat.

TABLE 4
Information Demand and Proprietary Costs

Panel A: Information Demand, Earnings Performance, and R&D Disclosure Quantity

Variable	Information Demand Proxy				
	Analysts	Institutional Ownership	Post Reg-FD	Earnings Forecasts	Poor Stock Returns
Intercept	0.211 [0.92]	0.200 [0.87]	0.179 [0.78]	0.191 [0.83]	0.183 [0.80]
adjROA	-0.020 [-0.77]	-0.029 [-1.07]	0.003 [0.10]	-0.027 [-1.09]	-0.012 [-0.44]
adjROA × Information Demand	-0.015*** [-2.74]	-0.115 [-1.26]	-0.103*** [-2.74]	-0.030*** [-3.10]	-0.046** [-2.14]
Information Demand	0.004** [2.39]	0.003 [0.08]	0.260 [1.08]	0.003 [1.28]	0.030*** [5.19]
Observations	22,482	22,482	22,482	22,482	22,482
Adjusted R ²	89.6%	89.6%	89.6%	89.6%	89.6%

Panel B: Proprietary Costs, Earnings Performance, and R&D Disclosure Quantity

Variable	Proprietary Cost Proxy			
	Product Substitutability	Ease of Entry	Industry Competition	GROWTH
Intercept	0.053 [0.20]	-0.897*** [-2.93]	0.086 [0.34]	0.294 [1.21]
adjROA	-0.224 [-1.09]	0.089 [0.48]	-0.006 [-0.12]	-0.062** [-2.01]
adjROA × Proprietary Costs	-0.143 [-0.88]	0.017 [0.77]	0.003 [1.23]	0.000 [0.22]
	-0.125 [-1.23]	-0.142*** [-5.26]	-0.009** [-2.12]	-0.004*** [-5.80]
Observations	22,315	22,315	20,675	21,560
Adjusted R ²	89.5%	89.6%	89.9%	89.5%

*, **, *** p < 0.1, p < 0.05, and p < 0.01, respectively.
This table examines whether the relation between R&D disclosure quantity and earnings performance is related to empirical proxies for information demand (Panel A) and proprietary costs (Panel B). Statistics on the control variables are not tabulated for presentation purposes. Regressions include time and industry fixed effects (based on Fama-French 48 industries) and use t-statistics based on standard errors that are robust to heteroscedasticity and clustered by firm.

Variable Definitions:
Analysts = number of analysts following the firm at the beginning of the fiscal period;
Institutional Ownership = percentage of institutional ownership at the beginning of the fiscal period;
Post Reg-FD = an indicator equal to 1 if the fiscal year-end follows the passage of Reg FD;
Earnings Forecasts = number of management forecasts issued during the fiscal period;
Poor Stock Returns = an indicator equal to 1 if the firm’s stock return over the fiscal year is negative;
Product Substitutability = firm’s industry gross margin (sales weighted average) multiplied by -1;
Ease of Entry = natural logarithm of the firm’s industry property, plant, and equipment (sales weighted average) multiplied by -1;
Industry Competition = firm’s industry four-firm concentration ratio multiplied by -1; and
Growth = ratio of the market value to book value of equity.

Earnings Performance and Other Characteristics of Narrative R&D Disclosure

I next consider other potential explanations for H1 (mentioned in Section II) related to opportunistic disclosure motives for a relation between R&D disclosure quantity and earnings performance. In general, these explanations relate to total disclosure quantity but are less likely to relate to disclosure detail. In fact, one could argue that these explanations might motivate firms to provide vague or “fluff” disclosures and even potentially to lower disclosure detail. In addition, managers might adjust R&D disclosure to put a positive spin on earnings news or to obfuscate performance information (e.g., Li 2008; Rogers, Van Buskirk, and Zechman 2011). Thus, I consider these issues by examining how earnings performance relates to measures of R&D disclosure detail, tone, and readability.

Measures of Disclosure Detail, Tone, and Readability

I include three proxies for disclosure detail based on prior work by Henry (2008), Li (2010a), and Brown and Tucker (2010). I measure the amount of numerical ($R\&D\ DISC_{NUM}$), forward-looking ($R\&D\ DISC_{FLS}$), and repetitive ($R\&D\ DISC_{REP}$) narrative R&D disclosure. I treat an R&D sentence as numerical if it contains numerical information that is not in the form of a date, such as December 31, 1997. I treat an R&D sentence as forward-looking if it contains future tense words as in Li (2010a).¹³ $R\&D\ DISC_{NUM}$ ($R\&D\ DISC_{FLS}$) is calculated as the number of R&D sentences with numerical (forward-looking) information. I also provide similar measures for earnings-related narrative disclosures ($EARN\ DISC_{NUM}$, $EARN\ DISC_{FLS}$) and non-R&D disclosures ($NonR\&D\ DISC_{NUM}$, $NonR\&D\ DISC_{FLS}$).

In addition, I measure the amount of repetitive or redundant R&D information. Psychology research finds that moderate amounts of repetition enhance communication, whereas greater amounts have negative effects (Cacioppo and Petty 1989). Following prior literature, I identify an R&D sentence as repetitive if it contains words very similar to another sentence in the same 10-K, based on a vector space model of text similarity (Salton, Wong, and Yang 1975). My measure is novel because I make comparisons within a single document as opposed to changes across documents as in Brown and Tucker (2010).

To compute this measure, I remove all articles and common prepositions from each sentence and reduce each word to its stem so that, for example, “stemming” and “stemmed” become “stem.” I then convert each sentence into an n -dimensional vector of word counts, where n is the total number of unique words found in a 10-K’s R&D sentences. I sequentially compare the vectors based on their cosine similarity to determine whether a sentence provides new information relative to prior sentences. The cosine similarity uses the following formula: $\cos(\theta) = \frac{V_1 \cdot V_2}{||V_1|| ||V_2||}$, where θ is the angle between vectors V_1 and V_2 , $(\cdot)$ is the dot product operator, and $||V_1||$ computes the length of the vector. I identify a sentence as repetitive if it is 90 percent or more similar to a previous sentence.¹⁴ I then compute $R\&D\ DISC_{REP}$ ($EARN\ DISC_{REP}$) as the total number of repetitive narrative R&D (earnings-related) disclosure sentences.

Following prior literature, I include measures that might capture opportunistic disclosure motivations. I measure the tone of each R&D disclosure sentence based on the frequency of positive and negative words, using the word lists from Henry (2008) and Henry and Leone (2010). I identify a sentence as positive or negative based on whether it has more positive or negative words.

¹³ This word list includes “will,” “could,” “should,” “expect,” “anticipate,” “plan,” “hope,” “believe,” “can,” “may,” “might,” “intend,” “project,” “forecast,” “objective,” and “goal.”

¹⁴ The cosine similarity is bounded between 0 and 1. I choose 0.90 as a threshold because I want to identify sentences that are very similar and most likely exact copies. The results are of the same direction and statistical significance when using lower thresholds such as 0.60 or 0.42 as in some computer applications (Penumatsa et al. 2006).

TABLE 5
Narrative R&D Disclosure Detail, Tone, and Readability

Variable	Mean	Median	Std. Dev.	% of Type
Quantity of Numerical Narrative Disclosure				
10-K R&D Disclosures	9.23	7	9.12	33.7%
10-K Earnings Disclosures	22.98	18	16.56	47.0%
Quantity of Forward-Looking Narrative Disclosure				
10-K R&D Disclosures	7.69	3	14.69	17.8%
10-K Earnings Disclosures	7.50	3	6.59	15.7%
Quantity of Repetitive Narrative Disclosure				
10-K R&D Disclosures	3.04	1	4.68	7.8%
10-K Earnings Disclosures	6.21	4	7.01	11.0%
Mean Tone of Narrative Disclosure				
10-K R&D Disclosures	0.21	0.20	0.18	
10-K Earnings Disclosures	0.02	−0.05	0.12	
Readability of Narrative Disclosure (Fog Index)				
10-K R&D Disclosures	23.57	23.30	3.06	
10-K Earnings Disclosures	18.62	18.30	2.78	

This table provides descriptive information about the measures of narrative R&D disclosure detail (numerical, forward-looking, and repetitive content), tone, and readability. An R&D disclosure sentence is identified as numerical if it contains numerical information that is not in the form of a date. An R&D disclosure sentence is identified as forward-looking if it contains specific future-related words and phrases based on Li (2010). An R&D disclosure sentence is considered repetitive if it contains words and phrases very similar (> 90 percent) to another sentence in the same 10-K based on a vector space of model of text similarity (Brown and Tucker 2010). R&D disclosure tone is computed as the number of positive sentences less negative sentences divided by total sentences following Li (2010). The sentences are categorized based on the frequency of positive and negative words (Henry and Leone 2010). R&D disclosure readability is measured with the Fog Index calculated using all of the R&D disclosures in a particular 10-K. The measures for 10-K earnings-related disclosures follow the same procedures. % of Type refers to the quantity of a particular R&D (earnings) disclosure detail measure divided by total R&D (earnings) disclosure quantity.

Following Li (2010a), I compute the overall narrative R&D disclosure tone for each 10-K document ($R\&D\ DISC_{TONE}$) as the number of positive sentences less the number of negative sentences divided by the total number of sentences. I measure the readability of narrative R&D disclosure ($R\&D\ DISC_{FOG}$) by using the Fog Index based on Li (2008), where $FOG = 0.40 \times (\text{Average Number of Words per Sentence} + \text{Average Number of Complex Words per Sentence})$. I provide similar measures for earnings-related disclosures ($EARN\ DISC_{TONE}$, $EARN\ DISC_{FOG}$). Prior studies have used these types of measures to examine managers’ incentives to spin reported results or to obfuscate information (e.g., Henry 2008; Li 2008; Rogers et al. 2011).

Table 5 provides descriptive statistics on the characteristics of narrative R&D disclosure and compares them with those of narrative earnings-related disclosures. I find that consistent with the difficulty of quantifying R&D activities, 10-Ks generally contain less numerical R&D information than numerical earnings information in narrative disclosures (mean of 9.23 sentences versus 22.98). Yet, 10-Ks contain similar amounts of forward-looking R&D and earnings-related information on average (mean of 7.69 sentences versus 7.50) but less R&D disclosure repetition (3.04 sentences versus 6.21). I find that narrative R&D disclosures contain more positive-tone sentences on average but are considerably less readable than earnings-related disclosures.

TABLE 6
Narrative R&D Disclosure Detail and Earnings Performance

Panel A: Numerical Disclosure Detail and Earnings Performance

	Variable	<i>Ln(R&D DISC_{NUM})</i>	<i>Ln(EARN DISC_{NUM})</i>
	Intercept	−0.252 [−1.04]	−0.401*** [−2.93]
Earnings Performance	<i>adjROA</i>	−0.012 [−0.41]	0.263*** [10.62]
Information Environment	<i>SIZE</i>	0.050*** [6.25]	0.033*** [5.10]
	<i>ANALYSTS</i>	0.004** [2.29]	0.000 [0.11]
	<i>%INST</i>	−0.005 [−0.14]	0.104*** [3.43]
	<i>MFCOUNT</i>	0.001 [0.43]	0.010*** [5.41]
	<i>Ln(NonR&D DISC_{NUM})</i>	0.246*** [19.00]	0.473*** [42.90]
	<i>AGE</i>	0.009 [0.39]	0.026** [2.30]
	<i>AGE × AGE</i>	0.000** [2.20]	−0.000 [−1.12]
Investment Type	<i>R&D/OPX</i>	1.856*** [11.81]	−0.406*** [−3.15]
	<i>R&D/OPX × R&D/OPX</i>	−1.329*** [−8.24]	0.167 [1.33]
	<i>BM</i>	0.077*** [6.43]	0.060*** [6.12]
	<i>CAPINT</i>	0.042 [0.68]	−0.153*** [−3.35]
Uncertainty	<i>RETVOL</i>	−0.018 [−0.58]	−0.063** [−2.07]
	<i>adjROAVOL</i>	0.039 [0.80]	−0.031 [−0.73]
Financing	<i>LEVERAGE</i>	0.048* [1.73]	−0.031 [−1.25]
	<i>STOCK_ISS</i>	0.022** [2.42]	−0.005 [−0.73]
Observations		22,482	22,482
Adjusted R ²		79.7%	78.9%

(continued on next page)

Regression Results

Table 6, Panel A provides the regression results of using numerical disclosure quantity as a proxy for disclosure detail. The first column provides the results of using numerical R&D disclosures. Although I find a negative coefficient on *adjROA*, it is not statistically significant. The lack of significance is perhaps not surprising given the difficulty of capturing R&D information in

TABLE 6 (continued)

Panel B: Forward-Looking Disclosure Detail and Earnings Performance

	Variable	<i>Ln(R&D DISC_{FLS})</i>	<i>Ln(EARN DISC_{FLS})</i>
	Intercept	−0.571** [−2.50]	−0.878*** [−5.78]
	Earnings Performance <i>adjROA</i>	−0.092*** [−3.03]	0.059* [1.90]
Information Environment	<i>SIZE</i>	0.047*** [5.43]	0.017** [2.14]
	<i>ANALYSTS</i>	0.004* [1.73]	0.001 [0.35]
	<i>%INST</i>	−0.030 [−0.68]	0.063* [1.67]
	<i>MFCOUNT</i>	−0.005* [−1.82]	0.014*** [6.10]
	<i>Ln(NonR&D DISC_{FLS})</i>	0.278*** [19.59]	0.476*** [36.95]
	<i>AGE</i>	0.005 [0.23]	0.008 [0.61]
	<i>AGE × AGE</i>	0.000* [1.86]	−0.000** [−2.56]
	Investment Type <i>R&D/OPX</i>	1.398*** [7.93]	−0.385*** [−2.68]
	<i>R&D/OPX × R&D/OPX</i>	−1.068*** [−6.08]	0.332** [2.22]
	<i>BM</i>	0.042*** [3.22]	0.048*** [3.79]
Uncertainty	<i>CAPINT</i>	−0.074 [−1.05]	−0.122** [−1.97]
	<i>RETVOL</i>	0.011 [0.32]	−0.076** [−2.01]
	<i>adjROAVOL</i>	−0.014 [−0.28]	−0.078 [−1.45]
Financing	<i>LEVERAGE</i>	0.043 [1.40]	0.026 [0.87]
	<i>STOCK_ISS</i>	0.010 [1.02]	0.009 [0.89]
Observations		22,482	22,482
Adjusted R ²		86.4%	72.2%

(continued on next page)

numbers beyond expenditure amounts. At a minimum, however, I find no evidence that managers adjust the numerical content downward as a function of earnings performance. I find a positive and significant coefficient on *adjROA* when the disclosure measure is based on earnings-related disclosures, suggesting that firms increase (decrease) the amount of numerical earnings disclosure as *adjROA* improves (declines).

Table 6, Panel B provides the results of using forward-looking disclosure quantity as a proxy for disclosure detail. The coefficient on *adjROA* for the R&D disclosure regression is negative and significant ($p < 0.01$), indicating that firms provide more (less) forward-looking R&D disclosure

TABLE 6 (continued)

Panel C: Narrative Disclosure Repetition and Earnings Performance

	Variable	<i>Ln(R&D DISC_{REP})</i>	<i>Ln(EARN DISC_{REP})</i>
Earnings Performance	Intercept	−0.624** [−2.45]	−0.541** [−2.29]
	<i>adjROA</i>	−0.076* [−1.82]	0.109*** [2.92]
Information Environment	<i>SIZE</i>	0.032*** [3.16]	0.033*** [3.27]
	<i>ANALYSTS</i>	0.003 [1.30]	0.001 [0.39]
	<i>%INST</i>	0.040 [0.79]	0.008 [0.18]
	<i>MFCOUNT</i>	0.001 [0.20]	0.001 [0.49]
	<i>Ln(NonR&D DISC_{REP})</i>	0.203*** [21.03]	0.430*** [43.63]
	<i>AGE</i>	−0.001 [−0.03]	−0.002 [−0.08]
	<i>AGE × AGE</i>	0.000*** [3.44]	0.000** [2.09]
	<i>R&D/OPX</i>	1.368*** [6.85]	−0.269 [−1.40]
	<i>R&D/OPX × R&D/OPX</i>	−0.956*** [−4.78]	−0.133 [−0.66]
Investment Type	<i>BM</i>	0.070*** [4.73]	0.045*** [2.89]
	<i>CAPINT</i>	0.016 [0.21]	0.064 [0.82]
	<i>RETVOL</i>	−0.051 [−1.14]	−0.016 [−0.38]
	<i>adjROAVOL</i>	−0.032 [−0.45]	−0.013 [−0.23]
Uncertainty			
Financing	<i>LEVERAGE</i>	0.100*** [2.91]	0.075** [2.17]
	<i>STOCK_ISS</i>	0.003 [0.27]	0.013 [1.13]
Observations		22,482	22,482
Adjusted R ²		69.0%	72.3%

*, **, *** p < 0.1, p < 0.05, and p < 0.01, respectively.
This table examines the relation between narrative R&D disclosure detail and earnings performance. The dependent variable in each panel is the natural logarithm of 1 plus the quantity of numerical, forward-looking, and repetitive disclosure sentences. Non-R&D disclosure measures are based on 10-K sentences not identified as R&D-related. All regressions include firm and time fixed effects and use t-statistics based on standard errors that are robust to heteroscedasticity and clustered by firm.

details as earnings performance declines (improves). This result suggests that firms provide information to help investors evaluate the future. The coefficient on *adjROA* for the earnings disclosure regression is positive and marginally significant, consistent with prior literature on earnings guidance (Miller 2002; Chen et al. 2011; Houston et al. 2010).

Table 6, Panel C provides the results based on the quantity of repetitive disclosure. I find only marginal evidence that R&D disclosure repetition is negatively related to earnings performance ($p < 0.10$), but this result is difficult to evaluate because the disclosure literature provides no guidance on how much repetition is helpful and how much is burdensome. In the case of R&D, managers might find it helpful to use repetition to emphasize important information. There might also be concerns about redundancy, limited attention, and the simple repetition of vague disclosure information (Financial Accounting Standards Board [FASB] 2012). If more (less) repetitive disclosure represents increased (decreased) disclosure detail, then this result is consistent with the information demand motivation of H1. Nonetheless, given the limited statistical significance of the result and the scarcity of prior literature on disclosure repetition, the implications are limited.

I next examine whether managers supply more (less) R&D disclosure as earnings performance declines (improves) in order to spin or obfuscate poor earnings information. I repeat the analyses of the regression in Equation (1), using the tone and readability of narrative R&D disclosure as dependent variables. The Table 7, Panel A results based on narrative disclosure tone show that current earnings performance relates positively to narrative R&D disclosure tone ($p < 0.05$). This evidence suggests that firms do not use R&D disclosures to put a positive spin on negative earnings performance.

Similarly, Table 7, Panel B provides results for narrative disclosure readability based on the Fog Index. To control for potential issues associated with a mechanical relation between the Fog Index and the number of sentences, I control for the total amount of disclosure and focus on how earnings performance relates to disclosure readability. After controlling for disclosure quantity, I find no evidence that earnings performance relates to the readability of R&D disclosures, but I do find evidence of obfuscation behavior for earnings-related narrative disclosures, which is consistent with Li (2008).¹⁵

Overall, the results based on the relation of earnings performance to other narrative disclosure characteristics are consistent with the information demand motivation of H1. Nevertheless, because I cannot conclusively reject all alternative explanations, I further support the information demand motivation through tests of H4 and H5, which examine whether the quantity of narrative R&D disclosure is useful to financial analysts and investors.

Narrative R&D Disclosure and Sell-Side Financial Analysts

H4 considers whether narrative R&D disclosure relates to the behavior of sell-side financial analysts, an important and sophisticated group of financial information users (Schipper 1991). I follow prior literature and examine how the quantity of narrative R&D disclosure relates to analyst following, earnings forecast accuracy, and earnings forecast dispersion (Barth et al. 2001; Jones 2007; Leavy et al. 2011).

I define analyst following as the number of analysts in the first I/B/E/S consensus annual earnings forecast after the 10-K filing. Forecast accuracy is the absolute value of the difference between the actual reported earnings and the most recent consensus after the 10-K, scaled by price three days prior to the date of the consensus. Forecast dispersion is the standard deviation of analyst forecasts in the most recent consensus following the 10-K filing, scaled by price three days prior to the date of the consensus. I follow the model used by Leavy et al. (2011) and include controls for

¹⁵ In addition, I replicate the results of Tables 6 and 7 by using a year-to-year changes model specification. In all cases, the sign and statistical significance of the coefficients is the same as in the firm-fixed effects models with one exception. I find no evidence that changes in *adjROA* relate to changes in narrative R&D disclosure tone.

TABLE 7
Narrative R&D Disclosure Tone, Readability, and Earnings Performance

Panel A: Narrative Disclosure Tone and Earnings Performance

	Variable	R&D DISC _{TONE}	EARN DISC _{TONE}
Earnings Performance	Intercept	0.100* [1.68]	0.060** [2.09]
	adjROA	0.020** [2.19]	0.029*** [3.94]
	SIZE	0.015*** [6.22]	0.005*** [2.82]
Information Environment	ANALYSTS	−0.000 [−0.32]	0.000 [0.04]
	%INST	0.001 [0.09]	−0.003 [−0.39]
	MFCOUNT	0.001* [1.75]	−0.000 [−0.70]
	NonR&D DISC _{TONE}	0.505*** [10.59]	0.880*** [28.62]
	AGE	−0.001 [−0.12]	−0.006** [−2.06]
	AGE × AGE	0.000*** [2.75]	0.000 [0.63]
Investment Type	R&D/OPX	0.102** [2.41]	−0.050 [−1.46]
	R&D/OPX × R&D/OPX	−0.090** [−2.27]	0.010 [0.30]
	BM	0.008* [1.88]	−0.008*** [−3.23]
	CAPINT	0.040** [2.15]	−0.094*** [−7.24]
Uncertainty	RETVOL	−0.039*** [−3.22]	−0.000 [−0.01]
	adjROAVOL	−0.046*** [−2.98]	0.021* [1.66]
Financing	LEVERAGE	0.001 [0.16]	−0.012 [−1.56]
	STOCK_ISS	0.001 [0.32]	0.002 [1.01]
Observations		22,482	22,482
Adjusted R ²		48.1%	41.0%

(continued on next page)

firm characteristics, where *GROWTH* is the year-to-year percentage sales growth, *SEGMENTS* is the natural log of the number of business segments, and *ADV* is the ratio of advertising expense to total operating expense. I follow Jones (2007) and include lagged dependent variables to further control for potentially omitted correlated variables. Regressions also include time and Fama-French 48 industry fixed effects. I report t-statistics based on standard errors that are robust to heteroscedasticity and clustered by firm.

TABLE 7 (continued)

Panel B: Narrative Disclosure Readability and Earnings Performance

	Variable	<i>R&D DISC_{FOG}</i>	<i>EARN DISC_{FOG}</i>
Earnings Performance	Intercept	16.011*** [12.30]	-5.458*** [-7.05]
	<i>adjROA</i>	-0.231 [-1.63]	-0.608*** [-4.13]
Information Environment	<i>SIZE</i>	-0.078* [-1.82]	-0.050 [-1.43]
	<i>ANALYSTS</i>	0.003 [0.25]	-0.006 [-0.70]
	<i>%INST</i>	-0.192 [-0.91]	-0.423*** [-2.67]
	<i>MFCOUNT</i>	-0.001 [-0.09]	-0.003 [-0.36]
	<i>NonR&D DISC_{FOG}</i>	0.408*** [14.33]	1.236*** [46.18]
	<i>SAME DISC_{QTY}</i>	0.707*** [7.73]	0.655*** [7.37]
	<i>AGE</i>	-0.159 [-1.34]	-0.095* [-1.66]
	<i>AGE × AGE</i>	0.001 [1.40]	0.000 [1.10]
Investment Type	<i>R&D/OPX</i>	-0.062 [-0.09]	-1.313* [-1.95]
	<i>R&D/OPX × R&D/OPX</i>	-0.379 [-0.55]	2.175*** [3.13]
	<i>BM</i>	-0.080 [-1.07]	-0.043 [-0.76]
	<i>CAPINT</i>	-0.179 [-0.56]	-0.042 [-0.15]
Uncertainty	<i>RETVOL</i>	-0.172 [-0.96]	0.196 [1.09]
	<i>adjROAVOL</i>	-0.407* [-1.83]	0.400 [1.60]
Financing			
	<i>LEVERAGE</i>	-0.192 [-1.18]	0.056 [0.40]
	<i>STOCK_ISS</i>	0.040 [0.75]	-0.006 [-0.14]
Observations		22,482	22,482
Adjusted R ²		46.2%	54.0%

*, **, *** p < 0.1, p < 0.05, and p < 0.01, respectively.
This table examines how narrative R&D disclosure tone and readability vary with earnings performance. Panel A examines disclosure tone. R&D disclosure tone is computed as the number of positive sentences less negative sentences divided by total sentences following Li (2010). The sentences are categorized based on the frequency of positive and negative words (Henry and Leone 2010). Panel B examines disclosure readability measured using the Fog Index. Non-R&D disclosure measures are based on 10-K sentences not identified as R&D-related. All regressions include firm and time fixed effects and t-statistics are based on standard errors that are robust to heteroscedasticity and clustered by firm.

The results in Table 8 show that narrative R&D disclosure quantity is positively related to subsequent analyst following, which is consistent with prior studies' findings that analysts prefer firms with better disclosure environments (Lang and Lundholm 1996). The results also suggest that R&D disclosure quantity is positively related to accuracy in the form of lower absolute forecast error and negatively related to forecast dispersion. This evidence suggests that analysts find narrative R&D disclosure informative, suggesting that R&D disclosure improves the quality of a firm's information environment.

Narrative R&D Disclosure, Information Content, and Information Asymmetry

H5 examines whether narrative R&D disclosure quantity relates to information content and information asymmetry. I follow prior studies and measure information content by using the absolute market-adjusted return ($AbRET$) in the $(-1, +1)$ window around the 10-K filing (Rogers and Van Buskirk 2009; Bushee, Core, Guay, and Hamm 2010). I also use the standardized return measure ($sAbRET$) from Cready and Hurtt (2002). For each day in the event window $(-1, +1)$, I take the absolute value of the size-adjusted return, subtract the mean absolute adjusted return from the pre-window $(-105, -6)$, and divide by the standard deviation of absolute adjusted returns from the pre-window. I then sum the standardized returns to compute $sAbRET$.

Similar to Rogers and Van Buskirk (2009), I include controls for the natural log of market value, book-to-market, and analyst following. I also control for the information content of the earnings announcement in the form of absolute market-adjusted returns in the $(-1, +1)$ window around the earnings announcement, changes in earnings ($\Delta adjROA$), and changes in R&D expenditures ($\Delta R\&D/OPX$). Additionally, I include firm fixed effects when $AbRET$ is the dependent variable to control for the normal level of absolute return and time fixed effects in both the $AbRET$ and $sAbRET$ regressions. The results in Table 9 show that the coefficient on $Ln(R\&D\ DISC_{QTY})$ is positive and significant in both regressions, suggesting that narrative R&D disclosure increases 10-K information content.

In addition, I examine whether higher R&D disclosure relates to reductions in information asymmetry around the 10-K as measured by changes in bid-ask spreads following the model of Rogers (2008). The dependent variable $\Delta SPREAD$ is the natural log of the average daily relative bid-ask spread in the $[+1, +10]$ window following the 10-K filing less the natural log of the average daily relative bid-ask spread prior to the filing $[-10, -1]$.¹⁶ I include controls for changes in the market variables following the same event windows, where $\Delta Std(Price)$ is the change in standard deviation daily stock price, $\Delta NumQuotes$ is the change in the number of quotes on the TAQ database, $\Delta \$Volume$ is the change in the natural log of average daily dollar volume, $ABRET_{EA}$ is the three-day size-adjusted return centered on the firm's earnings announcement, $ABRET_{10K}$ is the three-day size-adjusted return centered on the 10-K filing, $ABRET_{(-10,+10)}$ is the size-adjusted return over the event window, $Ln(Spread_{Before})$ is the natural log of the average daily relative bid-ask spread in the pre-10-K window $(-10, -1)$, and $Nasdaq$ equals 1 if NASDAQ is the firm's primary stock exchange. I also include firm and time fixed effects. All other variables are defined as in Table 1. The results in Table 10 show that the coefficient on $Ln(R\&D\ DISC_{QTY})$ is negative and significant, suggesting that narrative R&D disclosure reduces information asymmetry.¹⁷

¹⁶ The relative bid-ask spread is calculated as the average daily quoted value of the ask price minus the bid price divided by the midpoint of the bid and ask prices. Similar to prior studies, I only use quotes between 9:30 a.m. and 4:00 p.m. and remove quotes with relative spreads higher than 90 percent of the midpoint.

¹⁷ The relations between R&D disclosure, information content, and information asymmetry are similar in terms of direction and statistical significance when using measures of the year-to-year change in narrative R&D disclosure.

TABLE 8
Narrative R&D Disclosure Quantity and Sell-Side Financial Analysts

Variable	Following	Accuracy	Dispersion
Intercept	−2.939*** [−5.09]	0.016 [0.95]	0.012*** [3.60]
<i>Ln(R&D DISC_{QTY})</i>	0.070** [2.46]	−0.002** [−2.13]	−0.000** [−1.98]
<i>R&D DISC_{TOPE}</i>	0.027 [0.30]	−0.001 [−0.19]	−0.001 [−1.34]
<i>R&D DISC_{FOG}</i>	−0.004 [−0.70]	0.000 [0.44]	0.000 [1.63]
<i>Ln(NonR&D DISC_{QTY})</i>	−0.207*** [−4.73]	0.015*** [8.36]	0.002*** [5.93]
<i>NonR&D DISC_{TOPE}</i>	1.623*** [3.66]	−0.019 [−1.16]	−0.014*** [−3.99]
<i>NonR&D DISC_{FOG}</i>	0.057*** [3.54]	−0.001* [−1.74]	−0.000*** [−2.89]
<i>adjROA</i>	0.438*** [5.65]	−0.031*** [−5.11]	−0.018*** [−10.18]
<i>SIZE</i>	0.552*** [26.26]	−0.011*** [−18.25]	−0.002*** [−14.60]
<i>GROWTH</i>	0.245*** [10.23]	−0.002* [−1.84]	−0.001*** [−2.69]
<i>SEGMENTS</i>	−0.046* [−1.78]	−0.000 [−0.32]	−0.000 [−0.57]
<i>%INST</i>	0.449*** [5.20]	−0.023*** [−8.62]	−0.006*** [−8.95]
<i>MFCOUNT</i>	0.006 [0.71]	0.001*** [2.88]	0.000 [1.46]
<i>R&D/OPX</i>	0.149 [1.16]	0.012** [2.34]	0.010*** [6.77]
<i>ADV</i>	1.322* [1.83]	0.102*** [3.75]	0.027*** [4.60]
<i>RETVOL</i>	0.658*** [4.18]	0.002 [0.23]	0.010*** [3.49]
<i>Following_{t−1}</i>	0.786*** [107.57]		
<i>Accuracy_{t−1}</i>		0.327*** [12.87]	
<i>Dispersion_{t−1}</i>			0.274*** [10.41]
Observations	17,919	11,399	10,159
Adjusted R ²	89.0%	28.0%	33.6%

*, **, *** p < 0.1, p < 0.05, and p < 0.01, respectively.

(continued on next page)

TABLE 8 (continued)

This table examines how narrative R&D disclosure relates to analyst following, earnings forecast accuracy, and earnings forecast dispersion. Analyst following is the number of analysts contained in the first consensus annual earnings forecast following the 10-K filing. Forecast accuracy is the absolute value of the difference between the I/B/E/S actual reported earnings and the most recent consensus forecast following the annual report filing, scaled by price three days prior. Forecast dispersion is the standard deviation of analyst forecasts in the most recent consensus forecast following the annual report, scaled by price. *GROWTH* is the year-to-year percentage sales growth. *SEGMENTS* is the natural log of the number of business segments. *ADV* is the ratio of advertising expense to total operating expense. Regressions include time and Fama-French 48 industry fixed effects and use t-statistics based on standard errors that are robust to heteroscedasticity and clustered by firm.

TABLE 9
Information Content of 10-K R&D Disclosures

Variable	<i>AbRET</i>	<i>sAbRET</i>
Intercept	0.0385** [2.20]	-0.3525 [-0.93]
<i>Ln(R&D DISC_{QTY})</i>	0.0037** [2.40]	0.0631*** [2.87]
<i>R&D DISC_{TONE}</i>	0.0012 [0.35]	-0.1128 [-1.25]
<i>R&D DISC_{FOG}</i>	-0.0003 [-1.50]	0.0008 [0.15]
<i>Ln(NonR&D DISC_{QTY})</i>	0.0012 [0.54]	0.0709* [1.67]
<i>NonR&D DISC_{TONE}</i>	0.0205 [1.16]	0.6267 [1.37]
<i>NonR&D DISC_{FOG}</i>	0.0010 [1.64]	0.0067 [0.42]
<i>abs ABRET_{EA} </i>	0.0542*** [5.06]	-0.3923* [-1.81]
<i>ΔadjROA</i>	-0.0028 [-0.52]	-0.0833 [-0.67]
<i>Ln(MV)</i>	-0.0048*** [-3.48]	-0.0294** [-2.17]
<i>BM</i>	0.0008 [0.30]	0.0508 [1.17]
<i>ANALYSTS</i>	-0.0001 [-0.84]	-0.0050 [-1.39]
<i>Δ(R&D/OPX)</i>	-0.0034 [-0.39]	0.0289 [0.11]
Observations	18,635	18,635
Adjusted R ²	21.1%	0.7%

*, **, *** $p < 0.1$, $p < 0.05$, and $p < 0.01$, respectively.
This table examines whether the information content of 10-K filings relates to narrative R&D disclosure. The narrative R&D disclosure variables include measures of quantity, tone, and readability as described in Tables 2 and 5. The non-R&D measures are similarly calculated based on the non-R&D disclosures in the 10-K filing. The dependent variables are *AbRET*, the absolute value of the size-adjusted stock return in the window [-1, +1] centered on the 10-K filing date, and *sAbRET*, the standardized absolute return from Cready and Hurtt (2002). *ABRET_{EA}* is the three-day size adjusted return centered on the firm's earnings announcement. The first regression includes firm fixed effects and both regressions include time fixed effects. t-statistics are based on standard errors that are robust to heteroscedasticity and clustered by firm.
All other variables are defined as in Table 1.

TABLE 10
Narrative R&D Disclosures and Information Asymmetry

Variable	$\Delta SPREAD$
Intercept	-0.2531*** [-2.93]
$Ln(R\&D\ DISC_{QTY})$	-0.0228*** [-3.24]
$R\&D\ DISC_{TONE}$	-0.0085 [-0.48]
$R\&D\ DISC_{FOG}$	-0.0000 [-0.04]
$Ln(NonR\&D\ DISC_{QTY})$	0.0060 [0.69]
$NonR\&D\ DISC_{TONE}$	-0.0095 [-0.11]
$NonR\&D\ DISC_{FOG}$	0.0074*** [2.60]
$\Delta Std(Price)$	0.0076*** [3.65]
$\Delta NumQuotes$	0.0798*** [9.89]
$\Delta \$Volume$	-0.0902*** [-16.13]
$ABRET_{EA}$	0.0057 [0.25]
$ABRET_{10K}$	0.0035 [0.22]
$ABRET_{(-10,+10)}$	-0.2511*** [-6.81]
$\Delta adjROA$	-0.2555*** [-16.84]
$Ln(MV)$	-0.0499*** [-9.54]
BM	0.0014 [1.09]
$ANALYSTS$	-0.0015* [-1.81]
$\Delta(R\&D/OPX)$	0.0130 [0.41]
$Ln(Spread_{Before})$	-0.0868*** [-17.30]
$Nasdaq$	0.0889 [1.20]
Observations	18,624
Adjusted R ²	12.3%

*, **, *** p < 0.1, p < 0.05, and p < 0.01, respectively.

(continued on next page)

TABLE 10 (continued)

This table examines whether narrative R&D disclosure is related to changes in relative bid-ask spreads around the 10-K filing date. The dependent variable is the natural logarithm of the average daily relative bid-ask spread in the [+1, +10] window following the 10-K filing less the natural logarithm of the average daily relative bid-ask spread prior to the filing [-10, -1]. Changes in the market control variables are computed following the same event windows. The regression includes firm and time fixed effects and t-statistics are based on standard errors that are robust to heteroscedasticity and clustered by firm. All other variables are defined as in Table 1.

Variable Definitions:
 $\Delta Std(Price)$ = change in standard deviation daily stock price;
 $\Delta NumQuotes$ = change in the number of quotes on the TAQ database;
 $\Delta \$Volume$ = change in the natural log of average daily dollar volume;
 $ABRET_{EA}$ = three-day size adjusted return centered on the firm's earnings announcement;
 $ABRET_{10K}$ = three-day size adjusted return centered on the 10-K filing;
 $ABRET_{(-10,+10)}$ = size adjusted return over the event window;
 $Ln(Spread_{Before})$ = natural log of the average daily relative bid-ask spread in the pre-10-K window (-10, -1); and
 $Nasdaq$ = 1 if NASDAQ is the firm's primary stock exchange.

V. CONCLUSION

R&D activities are an important source of firm growth and value, but our understanding of firms' R&D disclosure decisions is limited. This study provides new insights about these decisions by showing that firms adjust their narrative R&D disclosures in response to changes in current earnings performance. I predict and find that earnings performance is negatively related to the quantity of narrative R&D disclosure. I attribute this result to firms adjusting R&D disclosure to provide relevant information in response to investors' changing information demands rather than to firms attempting to spin or obfuscate earnings information. Furthermore, I find no evidence that managers generally use this disclosure to put a positive spin on performance information or to obfuscate it.

In addition, I find that narrative R&D disclosure is positively related to analyst following and earnings forecast accuracy and negatively related to analyst forecast dispersion. I also find that greater narrative R&D disclosure is associated with higher 10-K information content and lower information asymmetry. These findings are consistent with economic theories that suggest that informative disclosures can benefit firms by alleviating problems relating to information asymmetry and adverse selection, which should ultimately benefit the firm through higher equity valuations and lower costs of capital (Healy and Palepu 2001). These findings also increase our understanding of firms' R&D disclosure choices and shed light on the ongoing debate over firms' disclosure choices for intangible investments (Lev 2001; Hand and Lev 2003).

In addition, my findings increase our understanding of firms' narrative disclosure decisions, an important disclosure channel used to convey contextual information about a firm's activities beyond financial statement numbers. Although my findings are not necessarily generalizable to other disclosures, the unique characteristics of the R&D disclosure setting make it useful to examine narrative disclosure decisions. Using this setting, I provide original evidence that earnings performance can affect distinct types of narrative disclosures differently. I find that earnings performance relates negatively to narrative R&D disclosure and positively to a general measure of narrative earnings disclosure. These findings suggest that narrative disclosures should not be generalized as a whole and emphasize for future research the importance of considering disclosure type in the formation of hypotheses and empirical tests.

REFERENCES

- Aboody, D., and B. Lev. 2000. Information asymmetry, R&D, and insider gains. *The Journal of Finance* 55 (6): 2747–2766.
- Bagnoli, M., and S. Watts. 2007. Financial reporting and supplemental voluntary disclosures. *Journal of Accounting Research* 45 (5): 885–913.
- Bamber, L., and Y. Cheon. 1998. Discretionary management earnings forecast disclosures: Antecedents and outcomes associated with forecast venue and forecast specificity choices. *Journal Accounting Research* 36 (2): 167–190.
- Barney, J. 1991. Firm resources and sustained competitive advantage. *Journal of Management* 17 (1): 99–120.
- Barth, M., R. Kasznik, and M. McNichols. 2001. Analyst coverage and intangible assets. *Journal of Accounting Research* 39: 1–34.
- Boone, J. P., and K. K. Raman. 2001. Off-balance sheet R&D assets and market liquidity. *Journal of Accounting and Public Policy* 20 (2): 97–128.
- Botosan, C., and M. Stanford. 2005. Managers' motives to withhold segment disclosures and the effect of SFAS No. 131 on analysts' information environment. *The Accounting Review* 80 (3): 751–771.
- Brown, S., and J. Tucker. 2010. Large-sample evidence on firms' year-over-year MD&A modifications. *Journal of Accounting Research* 49 (2): 309–346.
- Brown, S., K. Lo, and S. Hillegeist. 2009. The effect of earning surprises on information asymmetry. *Journal of Accounting and Economics* 47: 208–222.
- Bushee, B. 1998. The influence of institutional investors on myopic R&D investment behavior. *The Accounting Review* 73 (3): 305–333.
- Bushee, B., J. Core, W. Guay, and S. Hamm. 2010. The role of the business press as an information intermediary. *Journal of Accounting Research* 48 (1): 1–19.
- Cacioppo, J., and R. Petty. 1989. Effects of message repetition on argument processing, recall, and persuasion. *Basic and Applied Social Psychology* 10 (1): 3–12.
- Chan, L. K. C., J. Lakonishok, and T. Sougiannis. 2001. The stock market valuation of research and development expenditures. *The Journal of Finance* 56 (6): 2431–2456.
- Chen, S., D. Matsumoto, and S. Rajgopal. 2011. Is silence golden? An empirical analysis of firms that stop giving quarterly earnings guidance. *Journal of Accounting and Economics* 51 (1-2): 134–150.
- Cready, W., and D. Hurtt. 2002. Assessing investor response to information events using return and volume metrics. *The Accounting Review* 77 (4): 891–909.
- Dontoh, A. 1989. Voluntary disclosure. *Journal of Accounting, Auditing, and Finance* 4 (4): 480–511.
- Drake, M., D. Roulstone, and J. Thornock. 2012. Investor information demand: Evidence from Google searches around earnings announcements. *Journal of Accounting Research* 50 (4): 1001–1040.
- Entwistle, G. 1999. Exploring the R&D disclosure environment. *Accounting Horizons* 13 (4): 323–341.
- Ertimur, Y. 2004. *Accounting Numbers and Information Asymmetry: Evidence From Loss Firms*. Working paper, Duke University.
- Financial Accounting Standards Board (FASB). 2012. Disclosure framework. Available at: http://www.fasb.org/cs/BlobServer?blobkey=id&blobnocache=true&blobwhere=1175824166287&blobheader=application%2Fpdf&blobheadername2=Content-Length&blobheadername1=Content-Disposition&blobheadervalue2=768947&blobheadervalue1=filename%3DDDP_ITC_Disclosure_Framework.pdf&blobcol=urldata&blobtable=MungoBlobs.
- Frankel, R., W. Mayew, and Y. Sun. 2010. Do pennies matter? Investor relations consequences of small negative earnings surprises. *Review of Accounting Studies* 15 (1): 220–242.
- Franzen, L. A., K. J. Rodgers, and T. T. Simin. 2007. Measuring distress risk: The effect of R&D intensity. *The Journal of Finance* 62 (6): 2931–2967.
- Glassman, C. A. 2003. Speech delivered at Northwestern University. Available at <http://www.sec.gov/news/speech/spch041003cag.htm>

- Graham, J. R., C. R. Harvey, and S. Rajgopal. 2005. The economic implications of corporate financial reporting. *Journal of Accounting and Economics* 40: 3–73.
- Grandi, A., B. H. Hall, and R. Oriani. 2009. R&D and financial investors. In *Evaluation and Performance Measurement of Research and Development*, edited by V. Chiesa, and F. Frattini, 143–165. Cheltenham, U.K.: Edward Elgar.
- Griliches, Z. 1981. Market value, R&D and patents. *Economics Letters* 7 (2): 183–187.
- Griliches, Z. 1998. *R&D and Productivity*. Chicago, IL: Chicago University Press.
- Guo, R., B. Lev, and N. Zhou. 2004. Competitive costs of disclosure by biotech IPOs. *Journal of Accounting Research* 42 (2): 319–355.
- Hall, B. H. 1993. The stock market's valuation of R&D investment during the 1980s. *American Economic Review* 83 (2): 259–264.
- Hand, J., and B. Lev. 2003. *Intangibles: Values, Measures, and Risks*. Oxford, U.K.: Oxford University Press.
- Harris, M. S. 1998. The association between competition and managers' business segment reporting decisions. *Journal of Accounting Research* 36 (1): 111–128.
- Hayn, C. 1995. The information content of losses. *Journal of Accounting and Economics* 20 (2): 125–153.
- Healy, P. M., and K. G. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31: 405–440.
- Henry, E. 2008. Are investors influenced by how earnings press releases are written? *Journal of Business Communication* 45 (4): 363–407.
- Henry, E., and A. Leone. 2010. *Measuring Qualitative Information in Capital Markets*. Working paper, University of Miami.
- Houston, J. F., B. Lev, and J. W. Tucker. 2010. To guide or not to guide? Causes and consequences of stopping quarterly earnings guidance. *Contemporary Accounting Research* 27 (1): 143–185.
- Jaffe, A. B. 1986. Technological opportunity and spillovers of R&D: Evidence from firms' patents, profits, and market value. *American Economic Review* 76 (5): 984–1001.
- James, S., and J. M. Shaver. 2009. *Strategic Motivations for Voluntary Public R&D Disclosures*. Working paper, University of Minnesota and The Ohio State University.
- Jones, D. A. 2007. Voluntary disclosure in R&D-intensive industries. *Contemporary Accounting Research* 24 (2): 489–522.
- Joos, P., and G. A. Plesko. 2005. Valuing loss firms. *The Accounting Review* 80 (3): 847–870.
- Karuna, C. 2007. Industry product market competition and managerial incentives. *Journal of Accounting and Economics* 43: 275–297.
- Kimbrough, M. 2007. The influences of financial statement recognition and analyst coverage on the market's valuation of R&D capital. *The Accounting Review* 82 (5): 1195–1225.
- Koch, A., and A. Sun. 2004. Dividend changes and the persistence of past earnings changes. *Journal of Finance* 59 (5): 2093–2116.
- Kothari, S. P., T. E. Laguerre, and A. J. Leone. 2002. Capitalization versus expensing: Evidence on the uncertainty of future earnings from capital expenditures versus R&D outlays. *Review of Accounting Studies* 7 (4): 355–382.
- Lang, M., and R. Lundholm. 1993. Cross-sectional determinants of analyst ratings of corporate disclosures. *Journal of Accounting Research* 31 (2): 246–271.
- Lang, M., and R. Lundholm. 1996. Corporate disclosure policy and analyst behavior. *The Accounting Review* 71 (4): 467–492.
- Lansford, B. 2006. *Strategic Coordination of Good and Bad News Disclosures: the Case of Voluntary Patent Disclosures and Negative Earnings Surprises*. Working paper, The Pennsylvania State University.
- Leder, M. 2003. *Financial Fine Print: Uncovering a Company's True Value*. New York, NY: John Wiley & Sons, Inc.

- Lehavy, R., K. Merkley, and F. Li. 2011. The effect of annual report readability on analyst following and the properties of their earnings forecasts. *The Accounting Review* 86 (3): 1087–1115.
- Leuz, C. 2004. Proprietary versus non-proprietary disclosures: Evidence from Germany. In *The Economics and Politics of Accounting*, edited by C. Leuz, D. Pfaff, and A. Hopwood, 164–197. New York, NY: Oxford University Press.
- Lev, B. 1999. R&D and capital markets. *Journal of Applied Corporate Finance* 11 (4): 21–35.
- Lev, B. 2001. *Intangibles: Management, Measurement, and Reporting*. Washington, DC: Brookings Institution Press.
- Lev, B., and P. Zarowin. 1999. The boundaries of financial reporting and how to extend them. *Journal of Accounting Research* 37 (2): 353–385.
- Lev, B., and T. Sougiannis. 1996. The capitalization, amortization, and value-relevance of R&D. *Journal of Accounting and Economics* 21: 107–138.
- Li, F. 2008. Annual report readability, current earnings, and earnings persistence. *Journal of Accounting and Economics* 45 (2-3): 221–247.
- Li, F. 2010a. The determinants and information content of forward-looking statements in corporate filings—A naïve Bayesian machine learning approach. *Journal of Accounting Research* 48 (5): 1049–1102.
- Li, X. 2010b. The impacts of product market competition on the quantity and quality of voluntary disclosure. *Review of Accounting Studies* 15: 663–711.
- Lippman, S., and R. Rumelt. 1982. Uncertain imitability: An analysis of interfirm differences in efficiency under competition. *Bell Journal of Economics* 13: 418–438.
- Matsumoto, D., P. Maarten, and E. Roelofsen. 2011. What makes conference calls useful? The information content of managers' presentations and analysts' discussion sessions. *The Accounting Review* 86 (4): 1383–1414.
- Miller, B. 2010. The effects of reporting complexity on small and large investor trading. *The Accounting Review* 85 (6): 1487–1519.
- Miller, G. S. 2002. Earnings performance and discretionary disclosure. *Journal of Accounting Research* 40 (1): 173–204.
- Morricone, S., and R. Oriani. 2009. R&D information. In *Evaluation and Performance Measurement of Research and Development*, edited by V. Chiesa, and F. Frattini, 166–188. Cheltenham, U.K.: Edward Elgar.
- Nagar, V., D. Nanda, and P. Wysocki. 2003. Discretionary disclosure and stock-based incentives. *Journal of Accounting and Economics* 34: 283–309.
- Nichols, D. C. 2009. *Proprietary Costs and Other Determinants of Nonfinancial Disclosures*. Working paper, Cornell University.
- Penumatsa, P., M. Ventura, A. Graesser, M. Louwerse, X. Hu, Z. Cai, and D. Franceschetti. 2006. The right threshold value: What is the right threshold of cosine measure when using latent semantic analysis for evaluating student answers? *International Journal on Artificial Intelligence Tools* 15: 767–778.
- Previts, G. J., R. J. Bricker, T. R. Robinson, and S. J. Young. 1994. A content analysis of sell-side financial analysts company reports. *Accounting Horizons* 8 (2): 55–70.
- Rogers, J. 2008. Disclosure quality and management trading incentives. *Journal of Accounting Research* 46 (5): 1265–1296.
- Rogers, J., A. Van Buskirk, and S. Zechman. 2011. Disclosure tone and shareholder litigation. *The Accounting Review* 86 (6): 2155–2183.
- Rogers, J., and A. Van Buskirk. 2009. Shareholder litigation and changes in disclosure behavior. *Journal of Accounting and Economics* 47:136–156.
- Rogers, J., D. Skinner, and A. Van Buskirk. 2009. Earnings guidance and market uncertainty. *Journal of Accounting and Economics* 48 (1): 90–109.
- Rogers, R. K., and J. Grant. 1997. Content analysis of information cited in reports of sell-side financial analysts. *Journal of Financial Statement Analysis* 3 (1): 17–30.

- Sadka, G. 2004. *Financial Reporting and Product Markets: Learning from Competitors*. Working paper, Columbia University.
- Salton, G., A. Wong, and C. S. Yang. 1975. A vector space model for automatic indexing. *Communications of the ACM* 18: 613–620.
- Schipper, K. 1991. Analysts' forecasts. *Accounting Horizons* 5 (4): 105–121.
- Segerstrom, P. 1991. Innovation, imitation, and economic growth. *Journal of Political Economy* 99 (4): 807–827.
- Sougiannis, T. 1994. The accounting based valuation of corporate R&D. *The Accounting Review* 69 (1): 44–68.
- Verrecchia, R. E. 1983. Discretionary disclosure. *Journal of Accounting and Economics* 5: 179–194.
- Wolfe, R. M. 2010. *U.S. Businesses Report 2008 Worldwide R&D Expense of \$330 Billion: Findings From New NSF Survey*. NSF 10-322. Arlington, VA: National Science Foundation.

APPENDIX A

Appendix: <http://dx.doi.org/10.2308/accr-50649.s1>

Information Environment and the Investment Decisions of Multinational Corporations

Nemit Shroff

Rodrigo S. Verdi

Massachusetts Institute of Technology

Gwen Yu

Harvard University

ABSTRACT: This paper examines how the external information environment in which foreign subsidiaries operate affects the investment decisions of multinational corporations (MNCs). We hypothesize and find that the investment decisions of foreign subsidiaries in country-industries with more transparent information environments are more responsive to local growth opportunities than are those of foreign subsidiaries in country-industries with less transparent information environments. Further, this effect is larger when (1) there are greater cross-border frictions between the parent and subsidiary, and (2) the parents are relatively more involved in their subsidiaries' investment decision-making process. Our results suggest that the external information environment helps mitigate the agency problems that arise when firms expand their operations across borders. This paper contributes to the literature by showing that the *external* information environment helps MNCs mitigate information frictions *within* the firm.

Keywords: *investment; capital budgeting; multinational firms; cross-border frictions; agency frictions; transparency; financial reporting quality; information quality.*

JEL Classifications: *D83; G31; M41.*

We appreciate comments from John Harry Evans III (senior editor), Morton Pincus (editor), two anonymous referees, Karthik Balakrishnan (discussant), Beth Blankespoor, Brad Badertscher, John Core, Kevin Crosbie (from RavenPack), Srikant Datar, Iliia Dichev, Fritz Foley, Michelle Hanlon, Bob Kaplan, Russ Lundholm, Greg Miller, Jeff Ng, Krishna Palepu, Karthik Ramanna, Sugata Roychowdhury, Tatiana Sandino, Jayanthi Sunder (discussant), Karen Ton (discussant), Joe Weber, Hal White, seminar participants at the 2012 AAA Annual Meeting, Harvard Business School 2011 IMO Conference, 2012 FARS Conference, 2012 FEA Conference, 2012 ISB Conference, Purdue University, 2011 Toronto Accounting Research Conference, University of Oregon, and a Ph.D. workshop at the Massachusetts Institute of Technology. We also thank Patricia Naranjo for the excellent research assistance. We acknowledge financial support from the Division of Research at the Harvard Business School and the MIT Junior Faculty Research Assistance Program. We thank RavenPack for generously sharing the data on worldwide press coverage. All errors are our own.

Supplemental materials can be accessed by clicking the link in Appendix B.

Editor's note: Accepted by Morton Pincus.

Submitted: March 2012
Accepted: September 2013
Published Online: October 2013

Data Availability: Data are available from public sources identified in the paper.

I. INTRODUCTION

This paper studies the role of the information environment in helping multinational corporations (MNCs) monitor and evaluate their subsidiaries' investment decisions.¹ Prior research emphasizes that cross-border frictions increase information asymmetry between parents and their subsidiaries, which increases the cost of monitoring within MNCs. For example, cross-border frictions such as geographic and cultural differences make it difficult for parents to incentivize the subsidiary managers and to evaluate their actions (Roth and O'Donnell 1996). As a result, prior studies find that MNCs adjust their ownership structure, compensation contracts, and organizational design to mitigate these frictions (Desai, Foley, and Hines 2004; Antras, Desai, and Foley 2009; Siegel and Larson 2009). Despite empirical evidence of such "internal mechanisms" intended to mitigate cross-border frictions, substantial cross-sectional variation still exists in the efficiency of MNCs (Caves 2007), suggesting that these internal solutions are unlikely to fully resolve all information problems. Our paper builds on this literature by examining whether the *external* information environment serves as another mechanism for parents to monitor their subsidiaries' decisions and to deal with the information frictions within the firm.

The intuition for our hypothesis is that external information, such as that generated by other firms in the industry or by capital market participants, can be used to evaluate managerial actions. For example, Holmstrom (1979) shows that information disclosed by competitors and other related firms can be used to benchmark managers in relative performance compensation contracts. Holmstrom and Tirole (1993) show that external information aggregated in stock prices can be used as a monitoring mechanism to reduce agency problems, which can affect firm performance. Similarly, Bushman and Smith (2001) and Armstrong, Guay, and Weber (2010) discuss several cases in which external information is used to facilitate internal governance mechanisms that monitor and evaluate managers. Here again, the intuition is that external information complements internal governance systems when resolving agency problems.

We use the insights developed in the above literatures to study the role the external information environment plays in helping MNCs monitor a foreign subsidiary's investment decisions. We focus on investment because it is one of the most important managerial actions and the decision is observable. The external information environment refers to the quality and quantity of information produced by related firms, such as competitors and supply chain partners, and by information intermediaries, such as analysts and the business press, in the environment in which the subsidiary operates. The basic idea is that MNCs can use the information available in the subsidiary's operating environment to forecast product demand, evaluate the extent of competition, analyze the profitability and investment plans of direct competitors, etc. This information can then be used by MNCs to monitor and evaluate the subsidiary's investment decisions. As a result, we hypothesize that the investment decisions of subsidiaries located in rich information environments are more efficient than those of subsidiaries located in poor information environments. An implicit assumption underlying our prediction is that parents are involved in their subsidiaries' decision-making process either directly by participating in the decision process or indirectly by monitoring and evaluating their decisions. We explore this assumption in greater detail in our cross-sectional tests described below.

Our analyses exploit a novel dataset provided by ORBIS, which contains detailed information on the ownership structure of subsidiaries within MNCs. Our sample consists of 6,298 unique

¹ Throughout the paper, we use the terms "MNCs" and "parents" interchangeably.

foreign subsidiaries with 2,249 parent firms operating in 63 countries from 2000 to 2009, yielding 32,163 subsidiary-year observations. Following Wurgler (2000), Bekaert, Harvey, Lundblad, and Siegel (2007), and Badertscher, Shroff, and White (2013), we focus on the sensitivity of a subsidiary's investment to its growth opportunities. Prior research interprets a higher sensitivity as more desirable based on the idea that investment is more responsive to growth opportunities when the adjustment costs, such as information frictions and agency problems, are low (Hubbard 1998). We use asset growth as a proxy for investment and the price-to-earnings (*PE*) ratio measured at the country-industry level as a proxy for growth opportunities (Bekaert et al. 2007). Finally, to proxy for the quality of the external information environment, we use the median analyst coverage, press coverage, and the degree of earnings transparency by firms in the country-industry in which the subsidiary operates. These measures are extensively used as proxies for the quality of the information environment in prior research (Beyer, Cohen, Lys, and Walther 2010). We compute these measures at the country-industry-year level to capture the *external* information environment. By examining the quality of the external information environment, rather than the subsidiaries' own financial reporting and disclosure quality, we mitigate the concern that the information environment and subsidiary investment are endogenously determined.

Our main specification is a subsidiary-level regression of investment on growth opportunities and an interaction between growth opportunities and the quality of the external information environment. While widely used in prior research (Hubbard 1998; Bekaert et al. 2007), this model potentially suffers from a misspecification if there is measurement error in our proxy for growth opportunity and the measurement error systematically varies with the external information environment. To mitigate this concern, we include an extensive set of control variables. Specifically, we control for differences in the sensitivity of investment to growth opportunities across countries by including country fixed effects and an interaction between each country fixed effect and growth opportunities. As a result, our identification comes from cross-industry and time variation in the information environment *within* each country. We also include parent-subsidiary country-pair fixed effects to control for bilateral relationships between the parent's and subsidiary's countries (Dellestrand and Kappen 2012), MNC fixed effects to control for differences in investment policies across MNCs (Desai, Foley, and Hines 2009), and controls for other internal mechanisms that mitigate agency problems within the firm, including the parent's ownership structure, internal capital markets, and reliance on domestic banking credit.

Consistent with our hypothesis, we find that the sensitivity of subsidiary investment to growth opportunities is higher in country-industries with richer information environments. In economic terms, we find a 3 percent difference in investment between firms in rich and poor information environments for a one standard deviation change in growth opportunities. Given that the average investment in our sample is 18 percent,² this represents a relative difference of approximately 17 percent. These results provide initial support for our hypothesis that the information environment helps MNCs monitor and evaluate their subsidiaries' investment decisions.

Next, we perform two cross-sectional tests. First, our earlier findings suggest that the local information environment facilitates monitoring and ultimately mitigates cross-border frictions. To strengthen this result, we predict and find that the value of the local information environment is greater when there are greater cross-border frictions between the parent and subsidiary, for example, when the parent and the subsidiary are located in countries that speak different languages. Second, a key assumption underlying our main hypothesis is that the parents are involved in and actively monitor their subsidiaries' investment decision. However, MNCs vary in the degree to which investment decisions are made by managers from the parent firms and, as a result, the external

² Average investment is measured as the average asset growth for all subsidiaries in our sample (see Table 3).

information environment could have a differential role depending on the parent's involvement in the decision-making process. We explore this argument and find that our results are stronger when the parent is more involved in the subsidiary's investment decisions, for example, when the management team of the subsidiary is composed of a greater number of expatriates from the parent's country.

An important benefit of our cross-sectional test is that it exploits variation in the parent's location while holding the subsidiary's location constant. That is, we effectively compare the investment behavior of two subsidiaries *located in the same country* but vary in the levels of frictions between the parent and subsidiary. As a result, these tests further alleviate any concerns related to cross-country differences in the subsidiary's investing/institutional environment and measurement error in our proxies. This enhances the empirical identification of our inferences.

Finally, we perform a battery of sensitivity tests to verify the validity of our inferences. First, we use the adoption of International Financial Reporting Standards (IFRS) as a proxy for a time-series change in the quality of the external information environment (Barth, Landsman, and Lang 2008) and find that the sensitivity of investment to growth opportunities increases for subsidiaries located in IFRS adopting countries following the mandatory adoption of IFRS. In addition, we repeat our analyses using total factor productivity as an alternative proxy for investment efficiency (Schoar 2002; Giroud 2013). Finally, in the spirit of a difference-in-differences research design, we benchmark our results for foreign subsidiaries to those for domestic subsidiaries as well as to the results for local stand-alone firms operating in the same country as the foreign subsidiary because these benchmark firms do not face cross-border frictions. We find that our inferences are unaffected by these additional tests.

Two important caveats regarding our dataset should be noted. First, the ORBIS dataset exhibits significant cross-country variation in subsidiary coverage, primarily due to cross-country differences in the reporting requirements for private firms. As a result, subsidiaries in some countries are under-represented in our sample. In addition, ownership information in ORBIS is a stale variable, meaning that it is coded as of the latest year in the dataset. This potentially creates measurement error in our parent-subsidiary classification. We partially mitigate the influence of these database issues through our research design and sensitivity tests. Nonetheless, we acknowledge that such limitations of the ORBIS dataset may affect the generalizability of our findings.

Our paper is related to several streams of literature. First, it relates to both the financial and managerial accounting literatures. A recent stream of papers in financial accounting investigates how information quality affects corporate investment decisions (Hope and Thomas 2008; Biddle, Hillary, and Verdi 2009; Durnev and Mangen 2009; Chen, Hope, Li, and Wang 2011; Badertscher et al. 2013; Goodman, Neamtiu, Shroff, and White 2014). A separate stream of research in managerial accounting examines the role of internal control systems, such as incentive contracts and cost accounting systems, designed to facilitate managerial decision making and reduce agency costs (Anthony 1965; Bruns and Waterhouse 1975; Datar, Rajan, and Horngren 2013). For example, Chenhall (2003) illustrates how the type of internal controls systems used in a firm largely depends on the firm's external environment. Our paper links these two literatures by investigating how the external information environment can serve as a control system that reduces information frictions within MNCs.

Second, our study also contributes to the literature in economics and international business. Prior studies examine how firms deal with agency frictions when operating across borders and find that MNCs adjust their ownership structure, compensation contracts, and organizational design to mitigate these frictions (Zaheer 1995; Smith 2001; Desai et al. 2004; Antras et al. 2009; Siegel and Larson 2009). We extend this literature by exploring a unique dataset on subsidiary-level investment decisions by MNCs around the world. MNCs are an increasingly common organizational form and yet we know little about the factors driving their investment decisions,

in particular, the role of the external information environment in facilitating their investment. This is particularly important because MNCs face substantial cross-border frictions and the role of the information environment in resolving such frictions is potentially large. Our results suggest that the external information environment helps mitigate the agency problems that arise when firms expand their operations across borders.

II. PRIOR RESEARCH AND HYPOTHESES

Information Frictions within MNCs

The importance of information frictions within MNCs (and within multi-segment firms generally defined) becomes readily apparent when parents and subsidiaries are viewed within a principal-agent framework. Parent firms allocate resources across subsidiaries and therefore face the need to monitor their activities. However, increased information frictions and moral hazard within firms make resource allocation challenging for the parent (Stein 1997; Hope and Thomas 2008; Graham, Harvey, and Puri 2011). For example, information frictions can arise within multi-segment firms due to conflicting operational styles or corporate cultures across segments (Bushman, Chen, Engel, and Smith 2004). Further, unlike single segment firms, in multi-segment firms the individual business segments are shielded from takeover pressures (Cusatis, Miles, and Wooldridge 1993) and divisional managers are less likely to receive powerful equity incentives (Schipper and Smith 1986), leading to an increase in moral hazard.

The severity of such moral hazard and information frictions in multi-segment firms is likely to be exacerbated in MNCs as compared to domestic firms. Specifically, MNCs face cross-border frictions arising from geographic dispersion, cultural and language differences, differing legal systems, etc., which make it more difficult to monitor and/or incentivize divisional managers (Hamilton and Kashlak 1999; Mian 2006; Bell, Filatotchev, Rasheed 2012; Dellestrand and Kappen 2012). In response to these information frictions, prior studies show that MNCs seek to reduce the costs of monitoring subsidiaries by reallocating decision rights (Desai et al. 2004), sharing ownership with local partners (Antras et al. 2009), and improving information technology systems (Bloom, Sadun, and Van Reenen 2012). However, despite these internal mechanisms, there is still evidence of substantial cross-sectional variation in the efficiency of decision-making within MNCs (Caves 2007). For example, Mian (2006) shows that the foreign branches of multinational banks are less likely to lend to small, albeit fundamentally solvent, businesses because the profitability of such loans is difficult for foreign branches to communicate to the parent banks. We build on this line of research, but with an important distinction. In contrast to prior studies that focus on internal mechanisms, we investigate whether *external* sources, specifically the degree of transparency in the information environment in which the subsidiary operates, help MNCs monitor their subsidiaries' decisions and mitigate information frictions within the firm.

The Role of the External Information Environment

The idea that the external information environment provides information about firm performance dates back to the literature on relative performance evaluation. Holmstrom (1979) shows that external information from competitors can be used to benchmark managerial performance and improve compensation contracts.³ Holmstrom and Tirole (1993) show that external information aggregated in stock price can be used as a monitoring mechanism to reduce

³ More broadly, the literature on information transfer shows that competitors' disclosure activities affect investors' perceptions about related firms (Foster 1981).

agency problems and improve firm performance. We build on this literature by developing predictions about the role of the external information environment in monitoring managers' investment decisions. We focus on investment decisions because it is one of the most important managerial decisions and one of the most fundamental drivers of firm value (Hubbard 1998).

Prior studies argue that better information can improve investment because it allows shareholders to better monitor managerial actions (Bushman and Smith 2001; Chen, Goldstein, and Jiang 2007). Further, recent papers show that information about one firm can affect competitors' investments. Durnev and Mangen (2009) and Kedia and Philippon (2009) present evidence that a firm's misreporting activities, proxied by accounting restatements and fraud, can lead to sub-optimal investment by competitors due to their reliance on erroneous information. Sidak (2003) presents similar arguments by arguing that WorldCom's fraudulent disclosure and financial reports had negative real effects on other telecom firms, governments, and capital markets.⁴

We hypothesize that the information environment in which the foreign subsidiary operates, defined as the quantity and quality of information disclosed by peer firms and information intermediaries, can help MNCs monitor a subsidiary's investment decisions. This occurs because parents can gain insights about managerial actions by observing external information. For example, they can use external information to forecast the demand in the subsidiary's line of business, the extent of competition, and the profitability and investment outlays of direct competitors, all of which can be used to benchmark the actions and performance of subsidiary managers. In other words, a rich external information environment enables parents to better evaluate whether its subsidiary's investments are in line with the subsidiary's growth opportunities. This could then mitigate subsidiary managers' incentives to shirk and/or divert resources to seek private gain, leading us to our first hypothesis:

H1: The sensitivity of investment to local growth opportunities is higher for subsidiaries operating in more transparent information environments.

While we focus on the potential role played by the external information environment in resolving information frictions within MNCs and facilitating monitoring, an alternative hypothesis is that MNCs monitor subsidiary managers through other mechanisms. For example, Antras et al. (2009) find that MNCs often enter foreign markets through a wholly owned subsidiary when there is higher risk of misappropriation by the foreign subsidiary. If other mechanisms, such as ownership interest, allow MNCs to completely resolve the information frictions between the parent and subsidiary in a cost-effective manner, then the information environment would have little role to play in facilitating subsidiaries' investment decisions. Ultimately, it is an empirical question as to whether the external information environment facilitates monitoring within MNCs.

Cross-Sectional Predictions

In this section, we develop two predictions based on cross-sectional differences in the extent to which the external information environment facilitates subsidiaries' investment decisions. First, we hypothesize that the role of the external information environment in facilitating a subsidiary's investment decision is greater when there are greater cross-border frictions between the parent and subsidiary. The idea is that when cross-border frictions are high, such as when the parent and the subsidiary are geographically distant, the external information environment can play a greater role

⁴ Sidak (2003) finds that WorldCom's falsified reports and disclosures led to: (1) over-investment in network capacity, (2) the formulation of flawed government telecommunication policies, and (3) the sustained retrenchment of financing sources away from future telecom investment projects. See also Li (2012), Badertscher et al. (2013), and Beatty, Liao, and Yu (2013).

in disciplining subsidiary managers by providing parent firms with information about the local economic environment. This occurs because the value of external information that can be obtained from the local information environment, such as performance benchmarks and local investment opportunities, is relatively more important when the parent firm is more uncertain about the growth opportunities available to the subsidiary. In contrast, when cross-border frictions are low, the costs of exchanging information between parents and subsidiaries are similarly low, which likely results in greater information transfers from the subsidiary to the parent and *vice versa* (Grinblatt and Keloharju 2001; Di Giovanni 2005; Giroud 2013). Such information sharing allows the parent to better evaluate the context within which their subsidiary managers make their investment decisions with lesser reliance on the external information environment.⁵ The above discussion leads to our next hypothesis.

H2: The effect of the information environment on the sensitivity of investment to growth opportunities is greater when there are greater cross-border frictions between the parent and subsidiary.

Our second cross-sectional prediction is based on the allocation of investment decision-making rights between parents and subsidiaries. An important assumption in our main hypothesis is that the parents are involved in their subsidiaries' decision-making process. As a result, parents use the information available in their subsidiaries' operating environment to evaluate and monitor their investment decisions. However, prior research finds that some parents delegate their decision-making rights to subsidiary managers and design incentive schemes to align the subsidiary's interests with their own (Ghoshal and Nohria 1989; Ittner and Larcker 2001). In such a scenario, the external information environment is likely to play a relatively smaller role in helping parents monitor their subsidiaries, at least with respect to investment decisions.⁶ Based on the above argument, we hypothesize that the external information environment will be relatively more important in facilitating the subsidiary's investment decisions when the parent is more involved in the subsidiary's investment decision-making process.

H3: The effect of the information environment on the sensitivity of investment to growth opportunities is stronger when parents are more involved in the subsidiary's investment decision-making process.

Implicit in the development of the preceding two hypotheses is that the extent of cross-border frictions (H2) and the allocation of the investment decision rights (H3) are independent of each other. However, it is plausible that cross-border frictions affect the allocation of decision-making rights.⁷ In untabulated analyses, we find that the correlation between these constructs is small in magnitude (ranging from -0.001 to 0.184). Further, our inferences are unchanged when we control for decision-making rights in our test of cross-border frictions and *vice versa*. However, for the ease of exposition and to make our empirical models more tractable, we examine these hypotheses independently.

⁵ Note that if alternative internal mechanisms, such as ownership interest and the allocation of decision rights, fully resolve cross-border frictions within MNCs, then we would expect a null result for H2.

⁶ The alternative hypothesis is that the external information environment could still be used by the parent to incentivize the manager, e.g., through relative performance evaluation contracts. To the extent that such incentive mechanisms lead to more efficient investment, we would expect a null result for our third hypothesis.

⁷ For example, parent firms could retain greater control of the decision process when cross-border frictions are high, in an attempt to reduce agency costs. Alternatively, it is plausible that parent firms delegate decision-making rights to local managers when cross-border frictions are high to better exploit their knowledge of local growth opportunities in the subsidiary's country (i.e., there might be greater demand for local expertise).

III. SAMPLE SELECTION AND EMPIRICAL DESIGN

Data

We use the ORBIS database published by Bureau van Dijk (BvD), which includes ownership and financial information about public and private firms worldwide. BvD compiles information on public and private firms directly from annual reports and other well-established national data providers.⁸ We collect financial data for parents and subsidiaries from the 2010 CDs of the BvD industrial financial database. The majority of the subsidiaries report their financials using local accounting standards and approximately 2 percent of our sample firms report under IFRS. In addition to financial information, ORBIS provides additional firm-level information, including management and board member characteristics, industry classification, and country of domicile. A limitation of the ORBIS database is cross-country variation in subsidiary coverage due to the differences in the reporting requirements across countries.⁹ As a result, subsidiaries in some countries are under-represented in our sample. We address this limitation by studying within-country variation in subsidiary investment as we describe later.

We first construct the business group structure of MNCs by linking subsidiaries to parents using the BvD ownership database as illustrated in Appendix A. We define the ultimate parents as firms in which no single corporate shareholder owns more than 25 percent of the firms' shares, following the guidance in the ORBIS manual. Subsidiaries include firms both directly owned by the parent (Level 1) and subsidiaries indirectly owned via ownership of other subsidiaries (Levels 2, 3, and 4). We exclude subsidiaries with missing information on immediate ownership. This step reduces concerns about double-counting subsidiaries that appear multiple times at different ownership levels. We classify a business group as an MNC if it directly holds at least one subsidiary operating in a foreign country.¹⁰

We use all available data, subject to some minimal constraints. First, we exclude subsidiaries that are financial holding companies because they are less likely to invest in physical assets. We drop subsidiaries that lack the data to compute the growth opportunity measure that we describe below and subsidiaries with limited financial information in their unconsolidated financial statements.¹¹ All

⁸ The providers include WorldVest Base (WVB) and six regional data providers: EDGAR Online (U.S.), Huaxia International Business credit consulting company (China), Korea Information Service (Korea), Teikoku Databank (Japan), Reuters (U.S.), and Thompson Financial.

⁹ For example, in most European countries, both public and private companies are required to disclose annual financial data, while in North America only public firms are required to provide such information. As most subsidiaries are privately owned, North American subsidiaries are under-represented in the ORBIS database. Even within Europe there is cross-country variation in the reporting requirements of private firms. In some countries (e.g., Russia, Switzerland, and Ukraine), private firms are not required to provide financial information. In countries like Austria, private firms are required to provide only a few basic financial items.

¹⁰ One important limitation of the ORBIS database is that the ownership structure could be stale. That is, ORBIS only reports subsidiary information as of the database's most recent update. For example, if an MNC has no subsidiary in India before 2010, the most recent year in the database, but in 2010 it acquired an Indian firm that existed prior to 2010, we would erroneously treat the MNC as owning the Indian subsidiary for all years in our sample. Likewise, if a company had a subsidiary in India for the length of our sample but liquidated it in 2009, we would erroneously treat the MNC as not owning a subsidiary in India for all years in our sample. To assess whether this limitation affects our results, we follow Markle and Shackelford (2010) and select the last three years for which we have data as the cut-off for our sensitivity tests. The intuition is that because subsidiary ownership is likely to be sticky, there are likely to be fewer errors in the years immediately preceding 2010 than there would be earlier. In untabulated results, we find the coefficient for the interaction between *PE* and *IE* is positive and statistically significant, suggesting that our inferences are not significantly affected by this limitation.

¹¹ An exception is that when subsidiaries hold other Level 2 and 3 subsidiaries, we use the consolidated financial statements when the unconsolidated statements are not available. Due to this restriction, many subsidiaries in North America are excluded from our analysis.

TABLE 1
Sample Selection

Panel A: Sample Selection

	Observations Dropped	# Parent-Subsidiary- Year Observations
Parent-subsidiary-years with financials and industry codes		120,105
Excluding subsidiaries that are financial holding companies	31,245	88,860
Excluding subsidiaries located in country-industry-years with no <i>PEs</i>	22,938	65,922
Excluding domestic subsidiaries	33,759	32,163
Final MNC parent-subsidiary-year		32,163

Panel B: Firm-Year Observations

	Main Sample <i>Foreign Subsidiaries</i>	Control Sample <i>Domestic Subsidiaries</i>
# of parent-subsidiary-years	32,163	33,759
# of unique subsidiaries	6,298	6,019
# of unique parents	2,249	1,091

This table presents our sample selection procedure. We require firm-years to have data on net income for both the parent and the subsidiary. In addition, we require both parents and subsidiaries to have at least U.S.\$10,000 in assets and sales.

parents and subsidiaries are required to have at least U.S.\$10,000 in assets and sales to minimize outliers in computation of sales and asset growth. This leaves us with a sample of 65,922 parent-subsidiary-year observations. We then initially exclude domestic subsidiaries where cross-border frictions are absent but later use these domestic subsidiaries as a benchmark sample in Section V. These restrictions yield a sample of 32,163 parent-subsidiary-year observations from 2000 to 2009. Our final sample consists of 2,249 parents and 6,298 foreign subsidiaries spanning 63 countries. Table 1 describes our sample selection process.

Table 2, Panel A, shows the distribution of parents and subsidiaries by country. The distribution is unbalanced for both parent and subsidiary firms. For example, the United States is home to many parents but not many subsidiaries. Belgium, in contrast, has far more subsidiaries than parents, and Germany shows a high concentration of both parents and subsidiaries. Panel B shows the geographic distribution of subsidiaries by parent region. In other words, the table is structured so that each row (i.e., each parent region) adds up to 100 percent. A large proportion of parents are located in North America, with some significant representation in East Asia and Western Europe, while the majority of subsidiaries are located in Eastern and Western Europe. Among European parents, the high percentage in the diagonal of the matrix indicates that most subsidiaries are established within the parent’s region. This suggests that the preference for geographically proximate investments, which is well-documented in the equity home bias literature (Portes and Rey 2005), is also observed in our MNC sample. However, there is still substantial variation in subsidiary location. For example, for parents from South American countries, 10.6 percent of their foreign subsidiaries are in South America, 17.0 percent in North America, and 50.3 percent in Western Europe.

TABLE 2
Sample Composition by Country, Geographic Region, and Industry

Panel A: Sample Distribution by Country

Country	Parent		Subsidiary		Country	Parent		Subsidiary	
	# of Obs.	% of Obs.	# of Obs.	% of Obs.		# of Obs.	% of Obs.	# of Obs.	% of Obs.
Argentina (E)	2	0.0%	410	1.3%	Kuwait	49	0.2%	0	0.0%
Australia	330	1.0%	225	0.7%	Liberia	2	0.0%	0	0.0%
Austria	233	0.7%	30	0.1%	Luxembourg	100	0.3%	0	0.0%
Belgium	304	0.9%	2,345	7.3%	Malaysia (E)	62	0.2%	322	1.0%
Bermuda	149	0.5%	0	0.0%	Marshall Islands	7	0.0%	0	0.0%
Brazil (E)	29	0.1%	55	0.2%	Mexico (E)	28	0.1%	9	0.0%
Canada	403	1.3%	131	0.4%	Netherlands Antilles	21	0.1%	0	0.0%
Cayman Islands	44	0.1%	0	0.0%	New Zealand	42	0.1%	62	0.2%
Chile (E)	21	0.1%	0	0.0%	Norway	470	1.5%	613	1.9%
China (E)	5	0.0%	992	3.1%	Pakistan	5	0.0%	14	0.0%
Colombia (E)	0	0.0%	6	0.0%	Peru (E)	3	0.0%	40	0.1%
Croatia	16	0.0%	0	0.0%	Philippines (E)	0	0.0%	2	0.0%
Cyprus	36	0.1%	0	0.0%	Poland (E)	10	0.0%	810	2.5%
Czech republic (E)	5	0.0%	225	0.7%	Portugal	37	0.1%	470	1.5%
Denmark	529	1.6%	0	0.0%	Romania	24	0.1%	81	0.3%
Egypt (E)	2	0.0%	0	0.0%	Russia	15	0.0%	0	0.0%
Finland	717	2.2%	1,048	3.3%	Singapore (E)	157	0.5%	308	1.0%
France	2,293	7.1%	5,164	16.1%	Slovakia	2	0.0%	0	0.0%
Germany	2,068	6.4%	1,348	4.2%	Slovenia	25	0.1%	0	0.0%
Gibraltar	3	0.0%	0	0.0%	South Africa (E)	99	0.3%	22	0.1%
Greece (E)	50	0.2%	411	1.3%	Spain	624	1.9%	1,896	5.9%
Hong Kong (E)	33	0.1%	9	0.0%	Sri Lanka	6	0.0%	0	0.0%
Hungary (E)	0	0.0%	5	0.0%	Sweden	1,302	4.0%	1,635	5.1%
Iceland	12	0.0%	0	0.0%	Switzerland	0	0.0%	1	0.0%
India (E)	262	0.8%	457	1.4%	Taiwan (E)	243	0.8%	0	0.0%
Indonesia (E)	0	0.0%	74	0.2%	Thailand (E)	2	0.0%	926	2.9%
Ireland	172	0.5%	161	0.5%	Tunisia	5	0.0%	0	0.0%
Israel	216	0.7%	9	0.0%	Turkey (E)	3	0.0%	29	0.1%
Italy	230	0.7%	2,525	7.9%	United Kingdom	1,710	5.3%	6,824	21.2%
Jamaica	1	0.0%	0	0.0%	United States	12,108	37.6%	708	2.2%
Japan	6,481	20.2%	485	1.5%	Virgin Islands	15	0.0%	0	0.0%
Korea (South) (E)	341	1.1%	1,276	4.0%	Total	32,163	100%	32,163	100%

(continued on next page)

Research Design

Our main prediction is that the external information environments in which subsidiaries operate enable parents to better monitor their subsidiary’s actions, which affects the subsidiary’s investment decisions. To test this prediction, we examine whether the sensitivity of a subsidiary’s investment to its growth opportunities is affected by its information environment. Prior research interprets this sensitivity as a desirable feature of investment (Bekaert et al. 2007; Bushman, Piotroski, and Smith

TABLE 2 (continued)

Panel B: Distribution of Parents and Subsidiaries by Geographic Region

Parent\ Subsidiary	Africa	South East Asia	Middle East	East Asia	Oceania	South America	North America	Eastern Europe	Western Europe	Total (# Obs.)
Africa	0.0%	2.8%	0.0%	0.0%	3.7%	3.7%	4.7%	0.0%	85.0%	107
South East Asia	0.0%	16.2%	0.0%	9.7%	2.4%	0.0%	11.7%	1.0%	58.9%	494
Middle East	0.0%	1.8%	0.0%	0.0%	0.7%	0.0%	15.2%	3.2%	79.1%	282
East Asia	0.0%	12.5%	0.0%	15.0%	1.3%	0.7%	4.4%	4.6%	61.4%	7,103
Oceania	0.0%	10.8%	0.0%	5.5%	4.5%	0.3%	9.0%	11.9%	58.0%	379
South America	0.0%	3.2%	0.0%	5.4%	4.8%	10.6%	17.0%	8.7%	50.3%	312
North America	0.1%	4.7%	0.1%	7.9%	0.8%	2.2%	1.2%	13.0%	69.9%	12,511
Eastern Europe	0.0%	2.3%	0.0%	3.4%	0.2%	0.5%	0.7%	40.4%	52.5%	3,289
Western Europe	0.1%	5.4%	0.0%	6.6%	0.5%	1.8%	2.0%	19.3%	64.3%	7,686
Total	22	2,103	9	2,762	287	520	839	4,857	20,764	32,163

Panel A presents the distribution of both parent-year observations and subsidiary-year observations by country. (E) denotes countries classified as emerging markets (following Kwok and Reeb 2000). Panel B presents the distribution of both parent-year observations (vertical axis) and subsidiary-year observations (horizontal axis) by geographic region.

2011; Badertscher et al. 2013). The intuition is that investment is more responsive to investment opportunities when the adjustment costs are low (Hubbard 1998).¹²

To test our prediction, we estimate the following regression model using ordinary least squares (OLS) with subsidiaries indexed as *i*, parents as *m*, parents’ countries as *p*, subsidiaries’ countries as *s*, subsidiaries’ industries as *j*, and each year in the sample as *t*:

$$INV_{i,t} = \beta PE_{s,j,t} \times IE_{s,j,t} + \sum \beta_s PE_{s,j,t} \times Country_s + \sum \beta_k PE_{s,j,t} \times Internal_k + \sum \alpha_m MNC_m + \sum \alpha_{p,s} Country_{p,s} + \sum \alpha_j Industry_j + \sum \alpha_k Internal_k + Controls + \varepsilon_{i,t}, \tag{1}$$

where *INV* is a firm-level proxy for the subsidiary’s investment, *PE* is the price-to-earnings ratio used as a proxy for the subsidiary’s growth opportunities, *IE* is a proxy for the transparency of the external information environment, *Internal_k* is a set of internal mechanisms, *Controls* is a set of control variables associated with investment, *MNC_m* is fixed effects for each parent firm, *Country_{p,s}* is parent-subsidiary country-pair fixed effects, and *Industry_j* is industry fixed effects. The coefficient of interest is β , which captures the incremental sensitivity of investment to growth opportunities (*INV-PE*) in more transparent information environments. Our prediction is that subsidiaries in more transparent environments exhibit greater *INV-PE* sensitivity than subsidiaries in less transparent environments, consistent with H1: $\beta > 0$.

The three main variables in Equation (1) are investment, growth opportunities, and the information environment, discussed in the next section. Ideally, we would proxy for investment using capital expenditures and/or acquisitions. However, these data are not available for our sample of largely private subsidiaries. Thus, we proxy for subsidiary investment using the percentage change in total assets in a year (Badertscher et al. 2013). Following Bekaert et al. (2007), we use the price-to-earnings (*PE*) ratio of the country-industry-year in which the subsidiary operates as our

¹² Adjustment costs arise from information frictions such as adverse selection and moral hazard, among other things, leading to insufficient investment in growing projects and too much investment in declining projects. When adjustment costs are low, investment is more efficient because firms can more rapidly increase (decrease) investment in growing (declining) businesses/industries.

proxy for growth opportunities.^{13,14} Bekaert et al. (2007) point out that an advantage of using the industry *PE* ratio as a proxy for growth opportunities is that this measure is relatively exogenous to an individual firm's investment choices. We obtain monthly *PE* ratios from DataStream and annualize them using the median ratio in the calendar year.

Equation (1) includes a series of fixed effects intended to capture unobservable characteristics that affect subsidiary investment. First, because our coefficient of interest is the sensitivity of investment to growth opportunities, we include a series of interactions that could affect this relation. Specifically, in Equation (1), we allow the coefficient for $PE_{s,j,t}$ to vary by country by interacting $PE_{s,j,t}$ with indicator variables for each country where the subsidiary is located, yielding one estimated β for each country s . This is important because prior research finds that country-level institutional features, such as financial development and capital market integration, lead to differences in investment efficiency (Wurgler 2000; Bekaert et al. 2007). By including interaction terms between *PE* and country indicators, we control for the effect of country-level factors on investment efficiency. Further, measurement error in the *PE* ratio across countries could lead to biases in our inferences to the extent that the measurement error is correlated with differences in the information environment across countries (Erickson and Whited 2000). By allowing *INV-PE* sensitivities to vary by country, we effectively control for cross-country differences in measurement error in *PE* across countries and identify our effect of interest from within-country variation in the information environment.

Second, we control for a series of mechanisms that could affect investment and/or be used to monitor a subsidiary's decisions (labeled *Internal* and $PE \times Internal$). Specifically, we control for the parent firm's cash flow, because prior research finds that parent cash flows affect subsidiary investment through internal capital markets (Shin and Stulz 1998).¹⁵ We also control for the parent's ownership interest in the subsidiary because prior research finds that MNCs adjust their ownership to mitigate incentive problems between the parent and subsidiary (Antras et al. 2009).¹⁶ Finally, we control for the availability of local bank financing to control for additional bank monitoring that can affect the subsidiary's investment decisions. As ORBIS has very limited data on an individual subsidiary's bank loans, we use the total banking credit extended in the subsidiary's country (*Domestic Banking Credit*) to proxy for bank monitoring. *Domestic Banking Credit* is measured as the sum of all credit provided by the banking sector as a percentage of GDP. Finally, we note that although data limitations preclude us from directly controlling for all possible internal mechanisms, controlling for the interaction between *PE* and country indicators (discussed above) allows us to indirectly control for them as long as these mechanisms are largely driven by country-level factors.¹⁷

¹³ An alternative is to measure the *PE* ratio for each subsidiary. However, because most subsidiaries in our sample are not publicly traded, we are unable to measure subsidiary-specific *PE* ratios.

¹⁴ *PE* ratios can be interpreted as the price paid for a dollar of the firm's current earnings. Thus, when the riskiness of the earnings stream, accounting practices, the degree of market efficiency, etc., are held constant, the differences in *PE* ratios are likely to capture differences in available growth opportunities (Bekaert et al. 2007).

¹⁵ We use the cash flows obtained for the parent's consolidated financial statement as we do not have the financial statements on a parent-only basis.

¹⁶ Prior literature finds that the parent ownership percentage does not always reflect the control rights of the parent firm over the subsidiary, especially when MNCs are structured as family-controlled business groups (Claessens, Djankov, and Lang 2000). In untabulated analysis, we use (1) the position of each firm in the group, reflecting the number of firms that exist between the parent and subsidiary and (2) cash flow rights, measured as the sum of the minimum percentage ownership linking the parent and the subsidiary (La Porta, Lopez-de-Silanes, and Shleifer 1999) as alternative measures of control rights. Using these alternative measures of control rights yields similar inferences. Note that throughout the draft, the phrase "similar inferences" indicates that our results are statistically significant at the 10 percent level or better and the coefficients are of the same sign and similar magnitudes.

¹⁷ For example, Antras et al. (2009) and Robinson and Stocken (2013) find that country-level factors such as investor protection and financial development influence a number of MNC characteristics such as ownership (e.g., joint venture versus wholly owned subsidiaries), capital structure, organizational design (e.g., centralized versus decentralized management), etc.

We also control for a series of fixed effects that directly influence investment. First, we include indicator variables for each parent firm. This allows us to account for unobserved factors that affect investment decisions at the parent-firm level. For example, one could argue that certain MNCs are simply more successful in exploiting growth opportunities and that our results could reflect their preference for operating in more transparent environments. Including these indicator variables restricts the variation in subsidiary investment to within the MNC, thereby controlling for unobservable MNC-level factors that might affect investment.

Second, it is plausible that subsidiary investment is driven by the characteristics of the subsidiary's country relative to the parent's country, including differences in corporate tax rate, property rights, etc. For example, the U.S. offers much stronger property rights protection relative to India, which may cause Indian MNCs to conduct their R&D operations via subsidiaries located in the U.S. To control for such relative differences in country characteristics that might affect investment, we include indicator variables for each parent-subsidiary country-pair in our regressions. Third, we include industry fixed effects to capture differences in the industry characteristics of each subsidiary, such as production adjustment costs, that could affect investment. Finally, our set of *Controls* includes the subsidiary firm size (log of total assets) and performance (ROA) to control for subsidiary scale and profitability.

The External Information Environment

We use three proxies for the transparency of the external information environment (*IE*) based on (1) the amount of information produced by financial analysts, (2) the amount of information produced by the business press, and (4) the extent of earnings management by related firms.¹⁸ A notable feature is that these proxies are computed at the aggregate country-industry level to capture the *external* information environment, making them exogenous to a subsidiary's investment decisions.

Analyst coverage. Our first measure of the quality of the information environment is the number of analysts following the firms in each country-industry-year. Financial analysts collect, process, and disseminate information about firm performance and future outlook (Beyer et al. 2010; De Franco and Hope 2011). Prior research suggests that greater analyst coverage is related to higher reporting transparency (Lang and Lundholm 1996; Hope 2003; Lang, Lins, and Miller 2004), lower information uncertainty (Zhang 2006), and greater industry-wide information transfer (Piotroski and Roulstone 2004). The role of financial analysts as information intermediaries has been recognized in fields outside of accounting. For example, in the international business literature, Bell et al. (2012) suggest that financial analysts can help alleviate the cross-border frictions for firms operating in a foreign country.

We compute analyst following as the number of analysts following the firm. Firms without coverage in I/B/E/S are assumed to have no analyst coverage.¹⁹ We then compute the median

¹⁸ Our proxies for the information environment assume that increasing the quantity of information weakly increases information quality as investors and market participants can always ignore any additional information that is not relevant to them. This assumption is in line with prior disclosure theories. For example, Leuz and Verrecchia (2000, 91) note that "the theory is sufficiently broad as to allow the notion of 'increased levels of disclosure' to be interpreted as either an increase in the quantity of disclosure or an increase in the quality of disclosure (or both)."

¹⁹ We note that differences in I/B/E/S coverage across countries could affect the validity of this assumption. For example, in 2005, 28 percent (49 percent) of firms in Australia (Austria) have at least one analyst according to I/B/E/S. However, we do not know if the remaining firms missing coverage in I/B/E/S represent zero analyst coverage or I/B/E/S data biases. To reduce the possibility that our inferences are affected by such country-specific biases, we include country fixed effects in our regression specification so that we make inferences from within-country variations in investment.

number of analysts following the firms in each country-industry-year. We use the quartile rank of the country-industry analyst coverage every year as our first measure of *IE*.²⁰

Press coverage. Our second proxy for the quality of the external information environment is the amount of press coverage received by firms in a country-industry-year. Prior research finds that the pressure created by press coverage can play an important role in monitoring and disciplining firms. For example, Dyck, Volchkova, and Zingales (2008) find that press coverage affects the amount of corporate resources that are diverted to the sole advantage of the controlling shareholders. Miller (2006) finds that the press plays an important monitoring role by identifying accounting irregularities and by publicizing irregularities identified by other information intermediaries. Collectively, the evidence from prior research suggests that press coverage helps improve firms' information environments by identifying and disseminating a variety of corporate governance issues.

We use a large proprietary dataset of press coverage received by more than 28,000 firms over more than 11 years and 86 different countries, compiled by RavenPack. RavenPack collects this extensive dataset from different press sources, including news wires, disclosures to regulators, credit rating agencies, etc. We compute press coverage as the median number of articles about a firm in each country-industry-year. We use the quartile rank of the country-industry press coverage each year as our second measure of *IE*.²¹

Earnings transparency. Our final measure of the quality of the information environment is the degree of earnings transparency by firms in each country-industry. The idea is that if the accounting information surrounding a firm is less precise and conceals economic performance, then it can adversely affect the investment decisions of related firms. For example, Durnev and Mangen (2009) show that a firm's past misreporting activities, as proxied by restatement announcements, affects competitors' past investment decisions. Following prior literature, we use the magnitude of accruals relative to the magnitude of cash flows as a proxy for earnings management (Leuz, Nanda, and Wysocki 2003; Barth et al. 2008; Hope, Thomas, and Vyas 2013). We calculate this measure as the absolute value of accruals scaled by the cash flow from operations for the median firm in each country-industry-year. We multiply the measure by -1 and refer to it as earnings transparency for exposition. We use the quartile rank of country-industry each year as the final measure of *IE*.

IV. EMPIRICAL RESULTS

Descriptive Statistics

Table 3 presents the descriptive characteristics of the parents and foreign subsidiaries in our sample. Panel A shows the results for all firms. Untabulated results indicate that MNCs in our sample hold three foreign subsidiaries on average, which translates into 13.2 subsidiary-years. The average investment rate among subsidiaries is 18 percent of assets and the average *PE* is 19.6. The average asset size of subsidiaries in our sample is U.S.\$24.9 million and the average subsidiary *ROA* is 3.3 percent. Untabulated descriptives show that the parents are much larger in size with average assets of U.S.\$19.4 billion but they have a comparable mean *ROA* (3 percent). Panel A also shows that the average cash flow from operations (*CFO*) generated by parent firms is 8.6 percent of

²⁰ We use quartile ranks to reduce the measurement error in the information environment proxies and to ease interpretation. However, our inferences remain unchanged when we use a continuous version of our information environment proxies.

²¹ Shroff, Sun, White, and Zhang (2013) examine the overlap in monthly press release frequency between RavenPack and Factiva (an extensive press release database commonly used by researchers) for a random sample of 50 firms in the U.S. and find that the correlation in press-release frequency is 94.7 percent, which suggests that the database is fairly comprehensive.

TABLE 3

Descriptive Statistics of Multinational Firms at the Subsidiary Level

Panel A: Full Sample

Variable	Mean	Std. Dev.	P25	P50	P75	n
Number of Subsidiary-years per parent	13.15	18.73	4.00	7.00	15.00	2,249
Asset Growth (%)	17.92	53.98	−7.64	9.18	28.18	32,163
PE	19.63	17.00	11.35	15.65	21.25	32,163
Analyst Coverage	1.30	0.47	1.06	1.23	1.45	32,163
Press Coverage	0.51	0.29	0.25	0.50	0.75	32,163
Earnings Transparency	−0.61	0.28	−0.75	−0.50	−0.25	32,163
ROA (%)	3.32	23.92	−0.58	4.17	11.42	32,163
Log Assets	9.63	2.12	8.17	9.63	11.03	32,163
Parent CFO (% of assets)	8.62	9.70	4.63	8.57	13.04	32,163
Parent Ownership (%)	76.40	26.12	50.00	98.00	100.00	32,163
Domestic Banking Credit (% of GDP)	130.30	47.24	33.74	80.78	105.71	32,163

(continued on next page)

assets. The average ownership in its subsidiaries is 76.4 percent, which is partly driven by our sampling requirement that parents have at least 25 percent ownership interest in the entity for it to be included as a subsidiary.

Table 3, Panel B, presents the mean and standard deviation of *PE* and our proxies for *IE* by each country in our sample. The descriptive statistics are reported at the industry-year level (2,540 industry-years) in each country because we measure *PE* and *IE* at the industry level. The table shows that there is considerable variation in *PE* and in our *IE* measures within each country. For example, the average within-country standard deviation in analyst coverage (media coverage) for our sample of countries is 3.81 (1.56). This variation is important because our research design exploits only within-country variation in *IE* to explain *INV-PE* sensitivities. The table also shows that there is considerable variation in the number of industry-year observations. In untabulated analyses, we find that our inferences are unchanged if we restrict our sample to include only countries with at least 15 industry-year observations.

Regression Results: The Role of the Information Environment

Table 4 presents the results from estimating Equation (1). In Column 1, we present the baseline regression specification of *INV* on *PE* and control variables. To ease the interpretation of the coefficient for *PE*, this model excludes the interactions between the country fixed effects and *PE*. The table shows that the coefficient for *PE* is positive and statistically significant at the $p < 0.01$ level (Coeff. = 0.06, t-stat = 2.85). This result confirms the findings in Bekaert et al. (2007) for our sample and suggests that firms’ investment decisions are associated with industry-level *PE* ratios. In subsequent models, we control for the interaction between the country indicators and *PE*; therefore, the coefficient for *PE* is not directly interpretable as it captures the baseline relation between *PE* and *INV* in the country whose fixed effect is excluded from the regression. Therefore, we do not tabulate the coefficient for *PE* in our remaining analyses.

Columns 2–4 in Table 4 show that the coefficient for the interaction between growth opportunities and information environment ($PE \times IE$) is positive and statistically significant for all three measures of the information environment. Specifically, the coefficient for the interaction

TABLE 3 (continued)

Panel B: Descriptives of *PE* and *IE* by Country-Industries

Country	<i>PE</i>		<i>Analyst Coverage</i>		<i>Press Coverage</i>		<i>Earnings Transparency</i>		n
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
Argentina	12.05	12.27	0.43	1.28	1.53	0.97	−1.46	0.87	83
Australia	19.97	13.87	0.14	0.35	2.34	1.15	−1.02	0.36	67
Austria	22.79	24.38	3.37	6.27	3.05	2.03	−0.99	0.75	19
Belgium	17.97	16.36	3.12	4.18	2.07	1.39	−0.99	0.42	117
Brazil	25.86	29.93	1.33	2.40	1.99	1.35	−1.06	0.33	38
Canada	29.26	21.15	0.68	0.75	2.00	0.80	−0.93	0.41	60
China	38.91	21.73	0.00	0.00	1.92	0.77	−1.21	0.62	84
Colombia	49.77	53.45	0.00	0.00	2.00	1.26	−0.73	0.13	6
Czech Republic	37.88	44.55	0.74	1.83	1.35	0.74	−1.43	0.79	39
Finland	18.70	14.50	5.03	4.32	2.76	1.95	−0.72	0.23	88
France	17.24	12.04	0.98	1.83	2.74	1.67	−0.95	0.36	134
Germany	20.18	17.17	1.03	1.91	2.17	1.20	−1.14	0.42	101
Greece	23.19	23.40	2.13	4.99	2.30	1.49	−1.17	0.34	79
Hong Kong	22.48	5.61	0.00	0.00	1.56	0.53	−1.40	0.12	9
Hungary	6.86	2.10	6.60	7.09	3.10	2.25	−1.51	1.00	5
India	23.01	18.75	0.88	1.79	1.73	0.87	−0.98	0.43	92
Indonesia	13.01	9.01	0.31	0.85	1.60	0.90	−1.64	0.62	36
Ireland	12.89	6.70	2.11	2.23	1.89	1.01	−1.20	0.87	46
Israel	10.28	7.88	0.00	0.00	1.71	0.76	−1.00	0.22	7
Italy	23.40	21.20	3.27	4.53	1.92	1.18	−1.13	0.41	119
Japan	38.34	21.16	0.21	0.47	2.49	2.27	−0.94	0.15	92
Korea (South)	17.75	17.21	0.10	0.47	2.00	1.62	−1.15	0.52	111
Malaysia	16.03	20.66	0.12	0.46	1.70	1.02	−1.59	0.52	82
Mexico	13.93	1.94	1.17	2.24	1.11	0.33	−1.70	0.37	9
New Zealand	20.66	12.94	1.94	1.53	2.15	1.10	−0.99	0.56	27
Norway	29.85	32.89	4.66	6.98	1.98	1.37	−1.12	0.62	59
Pakistan	9.75	10.84	0.04	0.13	1.00	0.00	−0.88	0.63	14
Peru	32.07	27.54	0.28	0.74	1.46	0.64	−0.93	0.43	25
Philippines	11.10	0.21	0.00	0.00	2.25	0.35	−1.10	0.23	2
Poland	22.69	20.64	1.33	2.33	1.64	1.60	−1.46	0.90	89
Portugal	19.56	18.46	3.86	5.57	1.68	1.09	−1.24	0.91	74
Russia	22.54	23.90	2.48	3.00	1.61	0.75	−0.88	0.52	32
Singapore	20.46	23.74	1.24	3.68	1.51	0.63	−1.32	0.54	72
South Africa	14.64	7.61	0.75	0.95	1.48	0.82	−0.82	0.44	22
Spain	24.11	21.75	7.62	7.24	2.01	1.29	−0.87	0.42	128
Sweden	20.74	20.18	2.02	3.94	2.53	1.54	−0.99	0.46	104
Switzerland	20.30	—	9.50	—	5.50	—	−0.47	—	1
Thailand	17.76	17.55	0.41	0.94	2.30	1.66	−1.14	0.59	87
Turkey	19.46	16.65	0.48	1.50	1.27	0.57	−1.18	0.36	22
United Kingdom	17.96	12.34	1.02	1.52	2.23	0.83	−1.00	0.49	144
United States	23.49	11.04	0.72	1.02	5.07	2.35	−1.09	0.38	115
Total	21.84	20.37	1.79	3.81	2.18	1.56	−1.11	0.56	2,540

(continued on next page)

TABLE 3 (continued)

Panel A (B) in this table presents the firm-year-level (industry-year-level) descriptive statistics for the multinational firms in our sample (by country).

Variable Definitions:

- Asset Growth* = growth in assets for each subsidiary;
- PE* = price-to-earnings ratio for each subsidiary's country-industry-year;
- IE* = *Analyst Coverage*, *Press Coverage*, or *Earnings Transparency* as defined below;
- Analyst Coverage* = median number of analysts following firms in the country-industry-year;
- Press Coverage* = median number of press articles about firms in the country-industry-year;
- Earnings Transparency* = -1 times the absolute value of the ratio of accruals scaled by the cash flow from operations for the median firm in each country-industry-year;
- ROA* = return on assets, that is, net income by total assets, at the subsidiary level;
- Log Assets* = natural log of assets at the subsidiary level;
- Parent CFO* = cash flows from operations scaled by total assets for each parent (based on the parent's consolidated financial statements);
- Parent Ownership* = percentage of the subsidiary's stock that is owned by the parent. For subsidiaries that are held indirectly through other firms, we use the product of all observed percentage ownership connecting the subsidiary along each chain; and
- Domestic Banking Credit* = all credit provided domestically by the banking sector as a percentage of the Gross Domestic Product (GDP).

ranges from 0.19 to 0.23 and is statistically significant at the $p < 0.05$ level or better. In economic terms, a one standard deviation increase in growth opportunities (which equals 17 in Table 3, Panel A) translates as approximately a 4 percent increase in investment for firms in the top analyst coverage quartile and a 1 percent increase for those in the bottom quartile. That is, there is approximately a 3 percent difference in investment between firms in the highest and lowest analyst coverage quartile for a one standard deviation change in *PE*.²² Given that the average investment in our sample equals 18 percent, this represents a relative difference of approximately 17 percent.

In the final column in Table 4, we aggregate our three *IE* proxies by taking the average of their ranks. This procedure helps reduce measurement errors in the individual proxies. Consistent with the results in the previous columns, we find that the coefficient for $PE \times IE$ is positive and statistically significant at the $p < 0.01$ level. Overall, the results presented in Table 4 indicate that the sensitivity of investment to growth opportunities is higher for subsidiaries located in country-industries with more transparent information environments, consistent with H1.

Table 4 also shows that the coefficients for *ROA*, *Parent CFO*, and *Parent Ownership* are consistently positive and statistically significant, indicating that better-performing subsidiaries and subsidiaries whose parents have greater cash flows and ownership tend to invest more. Further, the coefficient for $PE \times \textit{Domestic Banking Credit}$ is also positive and significant, indicating that the availability of bank financing increases the *INV-PE* sensitivity. Together, the coefficients for *ROA* and $PE \times \textit{Domestic Banking Credit}$ suggest that financing constraints might be affecting subsidiaries' investments and that the availability of bank financing helps to partially mitigate this constraint.

²² Specifically, we obtain our estimate of economic significance by taking the product of the $PE \times IE$ coefficient, the standard deviation of *PE*, and the difference between the fourth and first quartiles of *IE*, where the quartile ranks are scaled by 4. In numeric terms, this equals $0.23 \times 17 \times (1 - 0.25)$.

TABLE 4
Investment Regressions Conditional on Information Environment

Variables	Pr. Sign	Dependent Variable: Asset Growth			
		Information Environment (IE) Measures			
		Analyst Coverage	Press Coverage	Earnings Transparency	Aggregate
PE	+	0.06*** (2.85)			
PE × IE	+	0.23*** (3.11)	0.19** (2.11)	0.23** (1.97)	0.52*** (3.25)
IE		−0.14*** (−4.90)	−0.05 (−1.64)	−0.05 (−1.56)	−0.18*** (−3.52)
ROA		0.31*** (7.28)	0.35*** (11.33)	0.35*** (11.33)	0.31*** (7.21)
Log Assets		−0.07*** (−22.84)	−0.07*** (−24.58)	−0.07*** (−24.56)	−0.07*** (−22.40)
Parent CFO		0.33*** (4.14)	0.14** (2.41)	0.13** (2.37)	0.27*** (2.72)
Parent Ownership		0.07*** (2.95)	0.06** (2.03)	0.06** (2.17)	0.06** (2.06)
Domestic Banking Credit		−0.25*** (−10.07)	−0.34*** (−10.92)	−0.36*** (−11.12)	−0.35*** (−11.97)
PE × Parent CFO			0.25 (1.10)	0.27 (1.16)	0.28 (1.24)
PE × Parent Ownership			0.06 (0.81)	0.06 (0.79)	0.05 (0.66)
PE × Domestic Banking Credit			0.68*** (6.03)	0.68*** (5.93)	0.72*** (6.74)
PE × Country Indicators	No	Yes	Yes	Yes	Yes
PE × Firm-Level Internal Mechanisms	No	Yes	Yes	Yes	Yes
MNC Indicators	Yes	Yes	Yes	Yes	Yes
Country Pairs Indicators	Yes	Yes	Yes	Yes	Yes
Industry Indicators	Yes	Yes	Yes	Yes	Yes
SE Clustering (Country-Industry)	Yes	Yes	Yes	Yes	Yes
# Firm-Years	32,163	32,163	32,163	32,163	32,163
Adj. R ²	13.2%	13.2%	13.1%	13.1%	13.2%

(continued on next page)

Cross-Sectional Results

Next, we examine our cross-sectional hypotheses, which predict differences in the importance of the role of the external information environment by (1) the level of cross-border frictions (H2) and (2) the extent to which the parent is involved in the subsidiary’s investment decision-making process (H3).

To test H2, we partition the sample into two groups based on the level of cross-border frictions between the parent and subsidiary and we estimate Equation (1) separately for the two subsamples. Following prior literature, we consider three proxies that capture cross-border frictions between the parent and subsidiary. The first proxy is based on the commonality of languages spoken in the

TABLE 4 (continued)

*, **, *** Denote statistical significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. Data in bold are the main variable of interest, indicating the incremental *INV-PE* sensitivity for subsidiaries located in country-industries with more transparent *IEs*. This table presents the results from subsidiary-level regressions of asset growth on industry growth opportunities, the information environments in each country-industry-year, an interaction term between growth opportunities and the information environment and control variables. All regressions include the fixed effects of each parent firm, parent country-subsidiary country-pair, and the subsidiary's ICB industry. The regressions also include interaction terms between each subsidiary-country indicator and *PE*. t-statistics are reported in parentheses below the regression coefficients and we cluster standard errors at the country-industry level.

Variable Definitions:

- PE* = price-to-earnings ratio of the country-industry-year of the subsidiary;
- IE* = yearly quartile rank of our proxies for the information environment. We proxy for the information environment using the median number of analysts following firms in the country-industry-year (*Analyst Coverage*), the median number of press articles about firms in the country-industry-year (*Press Coverage*), and -1 times the absolute value of the ratio of accruals scaled by the cash flow from operations for the median firm in each country-industry-year (*Earnings Transparency*);
- ROA* = net income scaled by total assets;
- Log Assets* = natural log of the subsidiaries' total assets;
- Parent CFO* = cash flows from operations obtained by the parent firm that owns the subsidiary;
- Parent Ownership* = percentage of the subsidiary's stock that is owned by the parent; and
- Domestic Banking Credit* = all credit provided domestically by the banking sector as a percentage of GDP.

parent's and subsidiary's countries. Sharing a common language reduces information asymmetry by reducing the transaction cost of exchanging information across borders. For example, Grinblatt and Keloharju (2001) find that investors prefer to hold equities in firms that share the investor's native tongue. Similarly, Di Giovanni (2005) examines cross-border acquisitions and finds that sharing a common language helps reduce the cost of doing business across borders. If the parent and subsidiary are incorporated in countries that share a common official language, we consider cross-border frictions to be low.²³

Our second proxy for cross-border frictions is based on the geographic distance between the parent and subsidiary. Prior studies interpret geographic distance as a measure of information acquisition cost and/or information asymmetry (Anderson and Van Wincoop 2004). Consistent with this interpretation, prior research finds that geographic distance accounts for much of the variation in the cross-border flows of both real goods and financial assets (Portes and Rey 2005; Mian 2006). We measure *Geographic Distance* as the distance between the parent's and subsidiary's countries of incorporation. If the geographic distance between the parent country's capital and the subsidiary country's capital is greater than the median distance in our sample, we consider the cross-border frictions to be high.

Our final proxy for cross-border frictions is based on the difference in operating risks in the parent's and the subsidiary's country. Prior studies argue that operating risks such as infrastructure risks, customer risks, political risks, etc., are higher in emerging markets than in developed economies (Kwok and Reeb 2000).²⁴ Thus, when MNCs from developed countries invest in

²³ An official language is defined as the primary and secondary language used in each country, according to *The World Factbook* (Central Intelligence Agency [CIA] 2011). For example, English is a common official language shared by the U.S. and India, which facilitates communication between managers in these countries. However, China and the U.S. do not have a common official language, making bilateral communication more difficult.

²⁴ For example, infrastructure risk includes the increased risk of transportation delays due to fewer highways, railways, less-developed air routes, and greater susceptibility to route closures, as well as a greater risk of telephone and power outages and delays in mail delivery (Kwok and Reeb 2000).

emerging economies their operating risk increases although the reverse does not increase operating risk. In the context of our study, an increase in operating risks implies higher cross-border frictions. Thus, we predict that the role of the information environment will be stronger when parents from developed countries invest in emerging economies.²⁵ We classify countries into developed and emerging markets following Kwok and Reeb (2000) and the International Monetary Fund's 1999 *Capital Market Report* (IMF 1999) (see Table 2, Panel A).

To better understand our cross-sectional tests, consider the following example. Both Italy and the U.K. are resident to parent firms that have subsidiaries in Canada. However, unlike the U.K., Italy does not share a common official language with Canada. Therefore, cross-border frictions between Italian parents and their Canadian subsidiaries are higher than the frictions between the U.K. parents and their Canadian subsidiaries. Our prediction is that, holding the subsidiary's location constant, the incremental *INV-PE* sensitivity due to higher *IE* in Canada is greater for subsidiaries owned by Italian parents than it is for the subsidiaries owned by U.K. parents. Therefore, this test compares the investment behavior of subsidiaries *located in the same country* and exploits variation in the parents' locations, thereby mitigating concerns arising from cross-country differences in the investing/institutional environment, such as cross-country differences in financial development, corruption, growth opportunities, etc.

Table 5 presents the results from estimating Equation (1) on high and low cross-border friction partitions. Although we tabulate results using an aggregate of our three *IE* proxies, our inferences are unchanged when we use individual *IE* proxies. We find that when cross-border frictions are high, the coefficient for $PE \times IE$ is positive and significant. For instance, the coefficient for $PE \times IE$ is positive and statistically significant when the parent and subsidiary are located in countries that do not share a common language (Coeff. = 0.55, t-stat = 3.34), but statistically insignificant when parents and subsidiaries share a common language (Coeff. = -0.76, t-stat = -1.27). The results using *Geographic Distance* and *Operating Risk* as proxies for cross-border frictions offer similar inferences. We also test and find that the coefficients $PE \times IE$ are statistically different from each other for high versus low cross-border friction subsidiaries.²⁶ Overall, the results in Table 5 are consistent with H2 and suggest that the information environment has a greater effect on the sensitivity of investment to growth opportunities when there are greater cross-border frictions between the parent and the subsidiary.

To test H3, we partition the sample based on the allocation of decision-making rights between the parent and subsidiary. Here again, we estimate Equation (1) separately for the two subsamples of subsidiaries classified based on the allocation of decision-making rights. We use three proxies to capture variation in the extent to which the parent is involved in the subsidiary's investment decision-making process. Our first proxy is the subsidiary's relative size based on total assets. Prior research finds that larger subsidiaries are more likely to have decision-making rights (Robinson and Stocken 2013). The size and scope of large subsidiaries make it more difficult for managers at the parent firm to obtain and process all the information needed to facilitate decision-making. Hence, decision-making in larger subsidiaries is more likely to be at the subsidiary level with less involvement from the parent. To measure a subsidiary's relative size within each MNC, we sort subsidiaries into two groups based on their total assets. We classify subsidiaries with above-median

²⁵ Following Kwok and Reeb (2000), we assume that the parent-subsidiary pairs operating within emerging (or developed) economies face similar operating risks and thus do not lead to increases in operating risk.

²⁶ We test for the difference in coefficients using a bootstrap test. Specifically, we randomly assign the distance classification to each observation and estimate model (1) for the pseudo high- and low-distance groups, respectively. We then compute a pseudo difference in coefficients for the high- and low-distance pairs. Repeating this procedure 1,000 times yields a null distribution of the difference in coefficients, which we use to test the significance of the difference in coefficients we report in Tables 5 and 6.

TABLE 5
Investment Regressions by Cross-Border Frictions between Parents and Subsidiaries
Dependent Variable: Asset Growth

Distance Measures	Pr. Sign	Common Language		Geographic Distance		Operating Risk	
		Low Friction	High Friction	Low Friction	High Friction	Low Friction	High Friction
PE × IE	+/+	-0.76 (-1.27)	0.55*** (3.34)	0.33 (1.50)	0.68*** (3.25)	0.44** (2.52)	0.78* (1.73)
IE		-0.07 (-0.32)	-0.18*** (-3.60)	-0.12** (-2.04)	-0.23*** (-3.75)	-0.17*** (-3.08)	-0.10 (-1.05)
ROA		0.12** (2.26)	0.35*** (8.38)	0.25*** (3.82)	0.34*** (6.84)	0.28*** (6.84)	0.70*** (5.43)
Log Assets		-0.14*** (-8.68)	-0.07*** (-22.33)	-0.08*** (-10.20)	-0.07*** (-17.68)	-0.08*** (-19.95)	-0.09*** (-6.52)
Parent CFO		(0.01)	0.35*** (3.06)	0.34*** (2.94)	0.20 (1.63)	0.23** (2.20)	0.63*** (3.02)
Parent Ownership		-0.02 (-0.14)	0.06** (2.07)	0.04 (0.85)	0.08** (2.12)	0.08** (2.39)	0.04 (0.48)
Domestic Banking Credit		-0.47*** (-7.15)	-0.33*** (-8.16)	-0.35*** (-7.16)	-0.34*** (-10.48)	-0.35*** (-11.00)	-0.53*** (-3.07)
PE × Parent CFO		0.57 (0.43)	0.23 (0.97)	0.21 (0.59)	0.34 (1.17)	0.38 (1.50)	-1.40*** (-2.81)
PE × Parent Ownership		-0.29 (-0.80)	0.05 (0.69)	-0.10 (-0.80)	0.17* (1.80)	0.04 (0.47)	0.10 (0.54)
PE × Domestic Banking Credit		1.83*** (2.90)	0.67*** (5.67)	0.65*** (3.05)	0.75*** (6.71)	0.76*** (6.23)	0.08 (0.16)
Test (PE × IE): High = Low		p-value: 0.001		p-value: 0.040		p-value: 0.099	
PE × Country Indicators		Yes	Yes	Yes	Yes	Yes	Yes
PE × Firm-Level Internal Mechanisms		Yes	Yes	Yes	Yes	Yes	Yes
MNC Indicators		Yes	Yes	Yes	Yes	Yes	Yes
Country Pairs Indicators		Yes	Yes	Yes	Yes	Yes	Yes
Industry Indicators		Yes	Yes	Yes	Yes	Yes	Yes
SE Clustering (Country-Industry)		Yes	Yes	Yes	Yes	Yes	Yes
# Firm-Years		4,088	28,075	12,380	19,783	27,557	4,606
Adj. R ²		17.0%	12.9%	13.9%	12.9%	12.9%	19.1%

*, **, *** Denote statistical significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. Data in bold are the main variable of interest, indicating the incremental INV-PE sensitivity for subsidiaries located in country-industries with more transparent IEs. This table presents the results from subsidiary-level regressions of asset growth on industry growth opportunities, the information environments in each country-industry-year, an interaction term between growth opportunities and the information environment and control variables. The regressions are partitioned into two groups based on the cross-border frictions between the parent and the subsidiary. All regressions include the fixed effects of each parent firm, parent-country subsidiary-country pair, and the subsidiary's ICB industry. The regressions also include interaction terms between each subsidiary-country indicator and PE. t-statistics are reported in parentheses below the regression coefficients and we cluster standard errors at the country-industry level.

(continued on next page)

TABLE 5 (continued)

Variable Definitions:

PE = price-to-earnings ratio of the country-industry-year of the subsidiary;

IE = average yearly quartile rank of our proxies for the information environment;

Common Language = an indicator variable that equals 1 if the parent and subsidiary are incorporated in countries that share a common official language. Official language is defined as the primary and secondary languages used in each country, according to the *World Fact Book* (CIA 2011);

Geographic Distance = an indicator variable that equals 1 if the geographic distance between the parent’s and subsidiary’s countries is greater than the median distance for our sample firms; and

Operating Risk = an indicator variable that equals 1 if the parent is incorporated in a developed country and the subsidiary in an emerging market country. Developed countries are defined following Kwok and Reeb (2000).

All other variables are as defined in Table 4.

total assets relative to the assets of other subsidiaries owned by the same MNC as having a more autonomous decision-making process.

Our second proxy exploits the nationality of the subsidiary’s management team and board members to identify the location of decision-making rights. These data are available in the ORBIS dataset. Parents are more likely to send expatriates to help manage the subsidiary when they wish to be involved in the subsidiary’s decision-making process. However, when local knowledge and relationships are important for decision-making, the subsidiary’s management team is more likely to be comprised of local managers who are native to the subsidiary’s country (Ahlstrom and Bruton 2010). Empirically, we classify subsidiaries whose management team has more (fewer) expatriates than the sample median as having higher (lower) parent firm involvement in the subsidiary’s decision-making processes.²⁷

Our final proxy for decision rights examines whether a subsidiary operates in the same industry as the parent firm. The international management literature shows that parents of MNCs are more likely to be involved in the subsidiary’s decision-making process when they possess greater knowledge of the project (Björkman, Barner-Rasmussen, and Li 2004). Following prior research, we assume that parent firm managers’ are likely to possess greater knowledge of the subsidiary’s operations when the subsidiary operates in the same industry as the parent (Goodman et al. 2014). We use the one-digit ICB code to classify the parent-subsidiary pairs into those operating in the same versus different industries.

Table 6 presents these results. Consistent with H3, the coefficient for $PE \times IE$ is positive and significant for the subsample of subsidiaries with low decision-making rights, but insignificant and smaller in magnitude for the subsample of more autonomous subsidiaries. These results suggest that the role of the information environment in facilitating subsidiary investment is greater when parents are more involved in their subsidiary’s decision-making.

Time-Series Variation in the Information Environment

Our main tests explore cross-sectional variation in the information environment. We now use IFRS adoption as a proxy for a time-series change in financial reporting standards that led to a significant change in the information environment. We then examine whether the investment decisions of subsidiaries located in IFRS-adopting countries improves following IFRS adoption.

²⁷ Expatriates are defined as members of the management team whose nationality is different from the country where the subsidiary is domiciled. We obtain the nationality of the individual managers from ORBIS. The management team is defined as all senior managers, C-suite executives in operations and R&D, and the board of directors. 28 percent of the managers in our sample have missing data on nationality in ORBIS. While we drop these observations in our main analysis, untabulated results show that the findings are robust to assuming the managers with missing nationality information to be local managers.

TABLE 6
Investment Regressions by Location of Decision Rights
Dependent Variable: Asset Growth

Allocation of Decision-Making Rights Between Parent and Sub	Pr. Sign	Relative Size		Management Team Composition		Operating Industry	
		Low Involve	High Involve	Low Involve	High Involve	Low Involve	High Involve
PE × IE	?/+	0.17 (0.92)	0.69*** (3.02)	0.23 (1.12)	0.77*** (3.90)	0.56** (2.10)	0.73*** (3.23)
IE		−0.16** (−2.36)	−0.16*** (−2.74)	−0.08 (−1.29)	−0.23*** (−3.33)	−0.14* (−1.94)	−0.26*** (−4.06)
ROA		0.36*** (5.59)	0.26*** (5.55)	0.39*** (6.18)	0.30*** (5.74)	0.33*** (5.03)	0.31*** (6.21)
Log Assets		−0.19*** (−15.10)	−0.18*** (−22.11)	−0.10*** (−10.15)	−0.08*** (−20.60)	−0.08*** (−17.18)	−0.09*** (−13.98)
Parent CFO		0.19 (1.59)	0.23* (1.66)	0.19 (1.40)	0.32** (2.42)	0.30*** (2.88)	0.25* (1.79)
Parent Ownership		0.07 (1.57)	0.08 (1.33)	0.05 (0.73)	0.09*** (2.62)	0.13*** (2.72)	0.07 (1.39)
Domestic Banking Credit		−0.31*** (−7.64)	−0.37*** (−8.13)	−0.37*** (−10.79)	−0.39*** (−6.79)	−0.35*** (−8.05)	−0.36*** (−10.38)
PE × Parent CFO		0.40 (1.47)	0.40 (1.07)	0.41 (0.72)	0.13 (0.46)	0.30 (1.01)	0.25 (0.69)
PE × Parent Ownership		−0.03 (−0.24)	0.04 (0.32)	0.15 (0.91)	0.04 (0.46)	−0.10 (−0.85)	0.19* (1.69)
PE × Domestic Banking Credit		0.60*** (4.75)	0.66*** (3.07)	0.88*** (5.44)	0.83*** (5.10)	0.69*** (3.83)	0.87*** (4.30)
Test (PE × IE): High = Low		p-value: 0.03		p-value: 0.05		p-value: 0.14	
PE × Country Indicators		Yes	Yes	Yes	Yes	Yes	Yes
PE × Firm-Level Internal Mechanisms		Yes	Yes	Yes	Yes	Yes	Yes
MNC Indicators		Yes	Yes	Yes	Yes	Yes	Yes
Country Pairs Indicators		Yes	Yes	Yes	Yes	Yes	Yes
Industry Indicators		Yes	Yes	Yes	Yes	Yes	Yes
SE Clustering (Country-Industry)		Yes	Yes	Yes	Yes	Yes	Yes
# Firm-Years		15,476	16,461	11,147	20,790	16,283	15,880
Adj. R ²		23.1%	23.4%	16.6%	12.4%	15.3%	13.4%

*, **, *** Denote statistical significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. Data in bold are the main variable of interest, indicating the incremental INV-PE sensitivity for subsidiaries located in country-industries with more transparent IEs. This table presents the results from subsidiary-level regressions of asset growth on industry growth opportunities, the information environments in each country-industry-year, an interaction term between growth opportunities and the information environment and control variables. The regressions are partitioned into two groups based on the location of decision rights between the parent and the subsidiary. All regressions include the fixed effects of each parent firm, parent-country subsidiary-country pair, and the subsidiary’s ICB industry. The regressions also include interaction terms between each subsidiary-country indicator and PE. t-statistics are reported in parentheses below the regression coefficients and we cluster standard errors at the country-industry level.

(continued on next page)

TABLE 6 (continued)

Variable Definitions:
PE = price-to-earnings ratio of the country-industry-year of the subsidiary;
IE = average yearly quartile rank of our proxies for the information environment;
Relative Size = an indicator variable that equals 1 if the subsidiary is above the median size (measured using total assets) relative to the other subsidiaries owned by the same parent firm;
Management Team Composition = an indicator variable that equals 1 if the subsidiary’s management team (i.e., all senior managers, C-suite executives in operations and R&D, and board of directors) has more expatriates than the sample median. Expatriates are defined as employees whose nationality differs from the country where the subsidiary is domiciled; and
Operating Industry = an indicator variable that equals 1 if the parent and the subsidiary operate in the same industry (one-digit ICB code).
All other variables are as defined in Table 4.

Prior research finds that voluntary and mandatory IFRS adoption is associated with higher earnings “quality” (Barth et al. 2008; Landsman, Maydew, and Thornock 2012) and a significant reduction in information asymmetry (e.g., Daske, Hail, Leuz, and Verdi 2008, 2013; Wahid and Yu 2013). Thus, IFRS adoption serves as another proxy for the richness of the external information environment, which complements the three measures we use in our main tests.

We examine the change in *INV-PE* sensitivities for subsidiaries located in IFRS-adopting countries using a difference-in-differences design. In contrast to prior studies that focus on the *firm adopting IFRS*, we use IFRS adoption as an event that triggers a change in the *external* information environment. Thus, we consider both mandatory as well as voluntary adopters of IFRS when constructing our proxy for IFRS adoption. For our first proxy, we obtain from Daske et al. (2008) the list of countries that mandatorily adopted IFRS in 2005 (with the exception of Singapore, which adopted IFRS in 2003) and include a dummy variable *Post 2005* for sample years post-2005 in Equation (1). In this model, we limit our sample to countries that have a low rate (< 25 percent) of early voluntary adopters because, to the extent a significant number of firms voluntarily adopt IFRS before 2005, the mandatory adoption date is less likely to serve as a significant shift in the external information environment.²⁸ For our second proxy, we include all IFRS-adopting countries but re-code the post-2005 variable to capture the percentage of firms adopting IFRS in a country over the sample period. For example, 26.4 percent to 36.1 percent of the firms in Germany voluntarily adopted IFRS between 2001 and 2004, before it was mandated in 2005. Therefore, we create a variable labeled *%IFRS* that takes on values ranging from 0.264 to 0.361 in the periods 2001 to 2004 and is set equal to 1 following mandatory adoption. We then focus on the difference-in-differences coefficient for countries with and without IFRS adoption (*PE* × *Post 2005* × *IFRS Country* and *PE* × *%IFRS* × *IFRS Country*). Note that, because our model includes an interaction between *PE* and each country, the interaction between *PE* and *IFRS Country* is absorbed by the country-*PE* fixed effects.

Table 7, Column 1 presents the results after excluding subsidiaries located in countries with more than 25 percent of the firms voluntarily adopting IFRS and column 2 uses the proportion of IFRS-adopting firms in a country to proxy for the change in the external information environment. Column 1 shows that the coefficient for *PE* × *Post 2005* × *IFRS Country* is positive and statistically significant (t-stat = 1.86). The magnitude of the coefficient (0.25) is smaller than what we report in Table 4, Column 5 (= 0.52), suggesting that the change in the *INV-PE* sensitivity pre- versus post-IFRS is smaller compared to the cross-sectional effect of our earlier proxies for the information

²⁸ Specifically, we exclude Austria, the Czech Republic, Germany, Hungary, Italy, Luxembourg, and Switzerland because more than 25 percent of the firms in these countries voluntarily adopted IFRS well before the mandatory adoption date. We obtain the rate of voluntary IFRS adoption from Daske et al. (2008).

TABLE 7
Using IFRS as a Shock to the Information Environment

Variables	Pr. Sign	Dependent Variable: Asset Growth	
		IFRS Proxy	
		Post 2005	%IFRS
<i>PE × IFRS Proxy</i>		−0.12 (−1.33)	−0.16* (−1.84)
<i>PE × IFRS Country</i>		9.65 (0.83)	9.42 (0.83)
<i>PE × IFRS Proxy × IFRS Country</i>	+	0.25* (1.86)	0.33*** (2.78)
<i>IFRS Proxy × IFRS Country</i>		−0.08** (−2.34)	0.02 (0.53)
<i>IFRS Proxy</i>		0.01 (0.45)	0.04 (1.45)
<i>ROA</i>		0.32*** (6.88)	0.31*** (7.23)
<i>Log Assets</i>		−0.07*** (−19.38)	−0.07*** (−23.01)
<i>Parent CFO</i>		0.23** (2.08)	0.23** (2.16)
<i>Parent Ownership</i>		0.04 (1.06)	0.05 (1.64)
<i>Domestic Banking Credit</i>		−0.25*** (−5.93)	−0.47*** (−10.00)
<i>PE × Parent CFO</i>		0.44 (1.23)	0.31 (1.00)
<i>PE × Parent Ownership</i>		0.20** (2.14)	0.11 (1.13)
<i>PE × Domestic Banking Credit</i>		0.53*** (3.35)	0.44*** (3.32)
<i>PE × Country Indicators</i>		Yes	Yes
<i>PE × Firm-Level Internal Mechanisms</i>		Yes	Yes
<i>MNC Indicators</i>		Yes	Yes
<i>Country Pairs Indicators</i>		Yes	Yes
<i>Industry Indicators</i>		Yes	Yes
<i>SE Clustering (Country-Industry)</i>		Yes	Yes
# Firm-Years		28,029	32,163
Adj. R ²		13.4%	13.3%

*, **, *** Denote statistical significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. Data in bold are our main variable of interest and denotes the difference-in-difference coefficient for countries with and without IFRS adoption. This table presents the results from subsidiary-level regressions of asset growth on industry growth opportunities, indicator variables for IFRS-adopting countries and the post-IFRS adoption years, an interaction term between growth opportunities and the IFRS indicator variables and control variables. *IFRS Country* is an indicator variable for all countries adopting IFRS. *IFRS Proxy* is either (1) *Post 2005*, which is an indicator variable for the calendar years after mandatory IFRS adoption in 2005 or (2) *%IFRS*, which is the proportion of firms adopting IFRS before its mandatory requirement (obtained from Daske et al. 2008). Following mandatory IFRS adoption, *%IFRS* is set equal to 1 for all observations. t-statistics are reported in parentheses below the regression coefficients and we cluster standard errors at the country-industry level. All other variables are as defined earlier (see Tables 3 and 4).

environment. Column 2 presents the results after considering countries that gradually changed to IFRS. The coefficient for $PE \times \%IFRS \times IFRS\ Country$ is also positive and statistically significant (t-stat = 2.78) with a similar magnitude to that found in Column 1. These results strengthen our inference that a richer external information environment facilitates the investment decisions of subsidiaries.

V. ADDITIONAL ANALYSES AND ROBUSTNESS TESTS

Analyses of Benchmark Firms

Our results thus far show that the local information environment is an important determinant of foreign subsidiaries' investment efficiency. Further, our results are stronger when cross-border frictions are high and when parents are more actively involved in a subsidiary's decision-making process. To further validate our inferences, we benchmark our findings for foreign subsidiaries to those obtained using two "placebo" samples where cross-border frictions are non-existent and the parent-subsidiary relationship is less applicable. First, we compare our findings for foreign subsidiaries to those for *domestic* subsidiaries of MNCs. Since domestic subsidiaries (as we define them) are located in the same country as the parent, cross-border frictions are absent. Second, we compare our findings for foreign subsidiaries to that for a matched sample of foreign stand-alone firms that operate in the same country-industry-year and are of similar size (measured using total assets) as the foreign subsidiary. The notion of parent-subsidiary relationships do not apply to stand-alone firms and by definition, these firms do not suffer from cross-border frictions. Therefore, we predict that the local information environment has a smaller effect on the investment decisions of domestic subsidiaries and foreign stand-alone firms operating in the same countries as our foreign subsidiaries.

We estimate Equation (1) for the sample of domestic subsidiaries and a modified version of Equation (1) for the sample of foreign stand-alone firms and compare these results with those for foreign subsidiaries.²⁹ Table 8 reports the results. To facilitate the comparison, we reproduce the results from Table 4 that estimate Equation (1) for the sample of foreign subsidiaries. The results show that the interaction between growth opportunities and the information environment is not significant among domestic subsidiaries (column 2) and foreign stand-alone firms (column 3). These results contrast with the positive and statistically significant coefficient for $PE \times IE$ for the sample of foreign subsidiaries. Further, the differences between the coefficients are statistically significant at the $p < 0.01$ level. Overall, these results are consistent with our hypotheses that the external information environment is an important driver of investment efficiency, especially when cross-border frictions are high and when the parent firms demand information because of their involvement in the subsidiary's investment decisions.

Untabulated Robustness Tests

We conduct two additional robustness tests that are tabulated and described in detail in an additional appendix that is available as a downloadable PDF file (please see Appendix B for the

²⁹ We modify Equation (1) for stand-alone firms by removing control variables related to the parent-subsidiary relationship. Specifically, we are unable to control for *Parent CFO*, *Parent Ownership*, and these variables interacted with *PE*. In addition, MNC indicator variables and parent-subsidiary pair indicator variables also do not apply. Finally, our sample size for stand-alone firms is smaller (31,957 observations) because we are unable to find a matching foreign subsidiary for some observations. In untabulated analyses, we find that our inferences from Table 4 are identical with this different regression specification and smaller subsample of matched foreign subsidiaries.

TABLE 8
Using Domestic Subsidiaries and Foreign Stand-Alone Firms as Benchmark Groups
Dependent Variable: Asset Growth

Benchmark Samples	Pr. Sign	Foreign Subsidiaries (Table 4)	Domestic Subsidiaries	Matched Foreign Standalone Firms
<i>PE × IE</i>	+/0/0	0.52*** (3.25)	-0.13 (-0.73)	-0.14 (-0.57)
<i>IE</i>		-0.18*** (-3.52)	-0.04 (-0.95)	0.07 (0.98)
<i>ROA</i>		0.31*** (7.21)	0.57*** (9.28)	0.36*** (8.36)
<i>Log Assets</i>		-0.07*** (-22.40)	-0.04*** (-12.08)	0.01** (2.04)
<i>Parent CFO</i>		0.27*** (2.72)	0.61*** (6.71)	NA
<i>Parent Ownership</i>		0.06** (2.06)	0.04 (1.53)	NA
<i>Domestic Banking Credit</i>		-0.35*** (-11.12)	-0.39*** (-9.35)	(0.07) (-1.29)
<i>PE × Parent CFO</i>		0.25 (1.11)	0.00 (-0.01)	NA
<i>PE × Parent Ownership</i>		0.06 (0.80)	0.16** (2.10)	NA
<i>PE × Domestic Banking Credit</i>		0.71*** (6.75)	0.92*** (5.51)	0.74*** (3.29)
Test (<i>PE × IE</i>): Control Sample = Treatment Sample			p-value: 0.007	p-value: 0.009
<i>PE × Country Indicators</i>		Yes	Yes	Yes
<i>PE × Firm-Level Internal Mechanisms</i>		Yes	Yes	Yes
<i>MNC Indicators</i>		Yes	Yes	NA
<i>Country Pairs Indicators</i>		Yes	Yes	NA
<i>Country Indicators</i>		N/A	N/A	Yes
<i>Industry Indicators</i>		Yes	Yes	Yes
<i>SE Clustering (Country-Industry)</i>		Yes	Yes	Yes
# Firm-Years		32,163	33,759	31,957
Adj. R ²		13.2%	12.0%	9.9%

*, **, *** Denote statistical significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. Data in bold are our main variable of interest, indicating the incremental *INV-PE* sensitivity for subsidiaries located in country-industries with more transparent *IE*s. This table presents the results from subsidiary-level regressions of asset growth on industry growth opportunities, the information environments in each country-industry-year, an interaction term between growth opportunities and the information environment and control variables. *PE* is the price-to-earnings ratio of the country-industry-year of the subsidiary. *IE* is the average yearly quartile rank of our proxies for the information environment. We proxy for *IE* using the median number of analysts following firms in the country-industry-year (*Analyst Coverage*), the median number of press articles about firms in the country-industry-year (*Press Coverage*), and -1 times the absolute value of the ratio of accruals scaled by the cash flow from operations for the median firm in each country-industry-year (*Earnings Transparency*). The regressions are estimated using three samples: (1) the foreign subsidiaries of MNCs, (2) the domestic subsidiaries of MNCs, and (3) foreign stand-alone firms that are matched to foreign subsidiaries on size, industry, country, and year. Domestic subsidiaries of MNCs are subsidiaries located in the same country as the parent. t-statistics are reported in parentheses below the regression coefficients and we cluster standard errors at the country-industry level. See Tables 3–5 for all variable definitions.

link). First, we examine whether our results are robust to controlling for additional factors that drive the MNC's initial decision to invest in a country. MNCs' investment decisions can be separated into two distinct stages: (1) the decision to first enter a country and (2) the decision to make ongoing investments conditional on entry. While our interest lies in the second stage, a potential concern with our tests is that our findings could simply reflect the role of transparency in the initial investment decision. More broadly, foreign investments made by MNCs are a subset of foreign direct investment (FDI) flows into a country and, thus, it is conceivable that our results are capturing the determinants of FDI documented in prior research. While our main tests include a series of fixed effects that capture many of the determinants of FDI, we also re-run our tests controlling for additional time-varying drivers of FDI, such as the level of corruption (Alesina and Weder 2002), GDP growth, the currency exchange rate (Froot and Stein 1991), and the interest rate differentials between the parent's and subsidiary's countries (Gross and Trevino 1996). We find that our results are robust to the addition of these control variables (see Table OA1 in the "Additional Tests" supplemental material file available in Appendix B).

Next, we examine whether our inferences are robust to using an alternative measure of investment efficiency—Total Factor Productivity (*TFP*). *TFP* is the portion of output not explained by the inputs used in a firm's production and, therefore, measures how efficiently and intensely the inputs (capital and labor) are utilized in the production process (Schoar 2002). We find that *TFP* is positively associated with all our transparency proxies (see Table OA2 in the "Additional Tests" supplemental material file available in Appendix B). These results suggest that our prior results on the relation between the information environment and the sensitivity of investment to growth opportunities manifest as higher productivity for subsidiaries located in more transparent information environments.

VI. CONCLUSION

We test whether the external information environment in which subsidiaries operate helps MNCs to monitor their subsidiaries' investment decisions. Prior research emphasizes that cross-border frictions lead to an increase in information frictions between parents and their subsidiaries, which creates a demand for information that parents can use to monitor and evaluate their subsidiaries. Based on this argument, we hypothesize that external information, such as that generated by other firms in the industry or by information intermediaries, can be used to enhance managerial investment decisions inside the firm.

We find robust evidence that the sensitivity of subsidiary investment to growth opportunities is higher in country-industries with better information environments. Further, the role of the information environment on the sensitivity of investment to growth opportunity is greater when (1) there are greater cross-border frictions between the parent and the subsidiary and (2) when the parent is more involved in its subsidiary's investment decision-making processes.

This paper contributes to the literature on the real effects of accounting information. While recent research examines whether and why the quality of information disclosed by a firm affects the disclosing firm's investment decisions, we examine the effects of firms' disclosures and information intermediary coverage on the investment decisions of peer firms. Our paper also contributes to prior research on MNCs' investment decisions and, more broadly, the literature on foreign direct investments. MNCs are unique because they are subject to frictions arising from operating in many different countries. MNCs deal with these cross-border frictions by altering contract design, sharing ownership with local partners, etc. Our findings suggest that the external information environment is another mechanism that helps mitigate cross-border frictions.

Our study exploits a novel dataset in the ownership structure of MNCs to provide preliminary evidence on the role of information in facilitating investment decisions in MNCs. While we have

described limitations of this dataset, it provides an important opportunity to study MNC behavior. Our paper leaves open a number of subsequent research opportunities. For example, given our findings, one can examine the other types of internal governance mechanisms, such as compensation schemes, that MNCs use to mitigate cross-border friction, and how such internal mechanisms interact with the local environment in the country where the subsidiary operates. Finally, our study focuses only on the investment behavior of MNCs. Future research can extend this line of inquiry by examining other outcomes such as organizational design, incentive systems, and financing decisions.

REFERENCES

- Ahlstrom, D., and G. Bruton. 2010. *International Management: Strategy and Culture in an Emerging World*. Mason, OH: South-Western Cengage Learning.
- Alesina, A., and B. Weder. 2002. Do corrupt governments receive less foreign aid? *American Economic Review* 92 (4): 1126–1137.
- Anderson, J., and E. Van Wincoop. 2004. Trade costs. *Journal of Economic Literature* 42 (3): 691–751.
- Anthony, R. 1965. *Planning and Control Systems: A Framework for Analysis*. Boston, MA: Harvard University Press.
- Antras, P., M. A. Desai, and C. F. Foley. 2009. Multinational firms, FDI flows, and imperfect capital markets. *The Quarterly Journal of Economics* 124 (3): 1171–1219.
- Armstrong, C., W. Guay, and J. Weber. 2010. The role of information and financial reporting in corporate governance and debt contracting. *Journal of Accounting and Economics* 50 (2-3): 179–234.
- Badertscher, B., N. Shroff, and H. White. 2013. Externalities of public firm presence: Evidence from private firms' investment decisions. *Journal of Financial Economics* 109: 682–706.
- Barth, M., W. R. Landsman, and M. H. Lang. 2008. International accounting standards and accounting quality. *Journal of Accounting Research* 46: 467–498.
- Beatty, A., S. Liao, and J. Yu. 2013. The spillover effect of fraudulent financial reporting on peer firms' investment efficiency. *Journal of Accounting and Economics* 55: 183–205.
- Bekaert, G., C. Harvey, C. Lundblad, and S. Siegel. 2007. Global growth opportunities and market integration. *Journal of Finance* 62: 1081–1137.
- Bell, R. G., I. Filatotchev, and A. A. Rasheed. 2012. The liability of foreignness in capital markets: Sources and remedies. *Journal of International Business Studies* 43: 107–122.
- Beyer, A., D. Cohen, T. Lys, and B. Walther. 2010. The financial reporting environment: Review of the recent literature. *Journal of Accounting and Economics* 50: 296–343.
- Biddle, G. C., G. Hillary, and R. S. Verdi. 2009. How does financial reporting quality relate to investment efficiency? *Journal of Accounting and Economics* 48 (2-3): 112–131.
- Björkman, I., W. Barner-Rasmussen, and L. Li. 2004. Managing knowledge transfer in MNCs: The impact of headquarters control mechanisms. *Journal of International Business Studies* 35 (5): 443–455.
- Bloom, N., R. Sadun, and J. Van Reenen. 2012. Americans do I.T. better. US multinationals and the productivity miracle. *American Economic Review* 102 (1): 167–201.
- Bruns, W. J., Jr., and J. H. Waterhouse. 1975. Budgetary control and organizational structure. *Journal of Accounting Research*: 177–203.
- Bushman, R. M., and A. J. Smith. 2001. Financial accounting information and corporate governance. *Journal of Accounting and Economics* 1-3: 237–333.
- Bushman, R. M., Q. Chen, E. Engel, and A. J. Smith. 2004. Financial accounting information, organizational complexity and corporate governance systems. *Journal of Accounting and Economics* 37: 167–201.
- Bushman, R. M., J. D. Piotroski, and A. J. Smith. 2011. Capital allocation and timely accounting recognition of economic losses. *Journal of Business, Finance and Accounting* 38 (1-2): 1–33.
- Caves, R. E. 2007. *Multinational Enterprise and Economic Analysis*. 3rd ed. Cambridge, U.K.: Cambridge University Press.
- Central Intelligence Agency. 2011. *The World Factbook*. Washington, DC: CIA. Available at: <https://www.cia.gov/library/publications/the-world-factbook/index.html>

- Chen, F., O. Hope, Q. Li, and X. Wang. 2011. Financial reporting quality and investment efficiency of private firms in emerging markets. *The Accounting Review* 86 (4): 1255–1288.
- Chen, Q., I. Goldstein, and W. Jiang. 2007. Price informativeness and investment sensitivity to stock price. *Review of Financial Studies* 20 (3): 619–650.
- Chenhall, R. 2003. Management control system design within its organizational context: Findings from contingency-based research and direction for the future. *Accounting Organizations and Society* 28: 127–168.
- Claessens, S., S. Djankov, and L. H. P. Lang. 2000. The separation of ownership and control in East Asian corporations. *Journal of Financial Economics* 58: 81–112.
- Cusatis, P., J. A. Miles, and R. J. Wooldridge. 1993. Restructuring through spin-offs. *Journal of Financial Economics* 33: 293–311.
- Daske, H., L. Hail, C. Leuz, and R. Verdi. 2008. Mandatory IFRS reporting around the world: Early evidence on the economic consequences. *Journal of Accounting Research* 46: 1085–1142.
- Daske, H., L. Hail, C. Leuz, and R. Verdi. 2013. Adopting a label: Heterogeneity in the economic consequences around IAS/IFRS adoptions. *Journal of Accounting Research* 51 (3): 495–547.
- Datar, S. M., M. Rajan, and C.T., Horngren. 2013. *Managerial Accounting: Decision Making and Motivating Performance*. Englewood Cliffs, NJ: Prentice Hall.
- De Franco, G., and O.-K. Hope. 2011. Do analysts' notes provide new information? *Journal of Accounting Auditing and Finance* 26 (2): 229–254.
- Dellestrand, H., and P. Kappen. 2012. The effects of spatial and contextual factors on headquarters resource allocation to MNE subsidiaries. *Journal of International Business Studies* 43 (3): 219–243.
- Desai, M., F. Foley, and J. R. Hines. 2004. The costs of shared ownership: Evidence from international joint ventures. *Journal of Financial Economics* 73 (2): 323–374.
- Desai, M., F. Foley, and J. R. Hines. 2009. Domestic effects of the foreign activities of US multinationals. *American Economic Journal: Economic Policy* 1 (1): 181–203.
- Di Giovanni, J. 2005. What drives capital flows? The case of cross-border M&A activity and financial deepening. *Journal of International Economics* 65: 127–49.
- Durnev, A., and C. Mangen. 2009. Corporate investments: Learning from restatements. *Journal of Accounting Research* 47: 679–720.
- Dyck, A., N. Volchkova, and L. Zingales. 2008. The corporate governance role of the media: Evidence from Russia. *The Journal of Finance* 63 (3): 1093–1135.
- Erickson, T., and T. Whited. 2000. Measurement error and the relationship between investment and Q. *Journal of Political Economy* 108: 1027–1057.
- Foster, G. 1981. Intra-industry information transfers associates with earnings releases. *Journal of Accounting and Economics* 3: 201–232.
- Froot, K., and J. Stein. 1991. Exchange rates and foreign investment: An imperfect capital markets approach. *Quarterly Journal of Economics* 106 (4): 1191–1217.
- Ghoshal, S., and N. Nohria. 1989. Internal differentiation within multinational corporations. *Strategic Management Journal* 10: 323–337.
- Giroud, X. 2013. Proximity and investment: Evidence from plant-level data. *Quarterly Journal of Economics* 128: 861–915.
- Goodman, T., M. Neamtiu, N. Shroff, and H. White. 2014. Management forecast quality and capital investment decisions. *The Accounting Review* 89 (1): 331–365.
- Graham, J., C. Harvey, and M. Puri. 2011. Capital allocation and delegation of decision-making authority within firms. NBER Working Paper No. w17370. Cambridge, MA: NBER.
- Grinblatt, M., and M. Keloharju. 2001. Distance, language, and culture bias: The role of investor sophistication. *Journal of Finance* 56: 1053–1073.
- Gross, R., and L. J. Trevino. 1996. Foreign direct investment in the United States: An analysis by country by origin. *Journal of International Business Studies* 27 (1): 139–155.
- Hamilton, R. D. III, and R. J. Kashlak. 1999. National influences on multinational corporate control system select. *Management International Review* 39 (2): 167–189.
- Holmstrom, B. 1979. Moral hazard and observability. *Bell Journal of Economics* 10: 74–91.
- Holmstrom, B., and T. Tirole. 1993. Market liquidity and performance monitoring. *Journal of Political Economy* 10: 678–709.

- Hope, O.-K. 2003. Disclosure practices, enforcement of accounting standards, and analysts' forecast accuracy: An international study. *Journal of Accounting Research* 41 (2): 235–272.
- Hope, O.-K., and W. Thomas. 2008. Managerial empire building and firm disclosure. *Journal of Accounting Research* 46: 591–626.
- Hope, O.-K., W. Thomas, and D. Vyas. 2013. Financial reporting quality of U.S. private and public firms. *The Accounting Review* 88 (5): 1715–1742.
- Hubbard, R. G. 1998. Capital-market imperfections and investment. *Journal of Economic Literature* 35: 193–225.
- International Monetary Fund (IMF). 1999. *Capital Market Report*. Available at: <http://www.imf.org/external/pubs/ft/icm/1999/>
- Ittner, C. D., and D. F. Larcker. 2001. Assessing empirical research in managerial accounting: A value-based management perspective. *Journal of Accounting and Economics* 32: 349–410.
- Kedia, S., and T. Philippon. 2009. The economics of fraudulent accounting. *Review of Financial Studies* 22: 2169–2199.
- Kwok, C. Y., and D. Reeb. 2000. Internationalization and firm risk: An upstream-downstream hypothesis. *Journal of International Business Studies* 31 (4): 611–629.
- La Porta, R., F. Lopez-de-Silanes, and A. Shleifer. 1999. Corporate ownership around the world. *Journal of Finance* 54: 471–517.
- Landsman, W. R., E. L. Maydew, and J. R. Thornock. 2012. The information content of annual earnings announcements and mandatory adoption of IFRS. *Journal of Accounting and Economics* 53: 34–54.
- Lang, M., and R. Lundholm. 1996. Corporate disclosure policy and analyst behavior. *The Accounting Review* 71: 467–492.
- Lang, M., K. Lins, and D. Miller. 2004. Concentrated control, analyst following, and valuation: Do analysts matter most when investors are protected least? *Journal of Accounting Research* 42: 589–623.
- Leuz, C., and R. E. Verrecchia. 2000. The economic consequences of increased disclosure. *Journal of Accounting Research* 38: 91–124.
- Leuz, C., D. Nanda, and P. Wysocki. 2003. Earnings management and investor protection: An international comparison. *Journal of Financial Economics* 69: 505–527.
- Li, V. 2012. *Do False Financial Statements Lead Peer Firms to Make Distorted Decisions? An Analysis of Peer Firms' Real Operating Decisions during Periods of Misreporting*. Working paper. Available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2172074
- Markle, K. S., and D. A. Shackelford. 2010. *Do Multinationals or Domestic Firms Face Higher Effective Tax Rates?* Working paper, The University of North Carolina.
- Mian, A. 2006. Distance constraints: The limits of foreign lending in poor economies. *Journal of Finance* 61 (3): 1465–505.
- Miller, G. 2006. The press as a watchdog for accounting fraud. *Journal of Accounting Research* 44: 1001–1033.
- Piotroski, J. D., and D. T. Roulstone. 2004. The influence of analysts, institutional investors, and insiders on the incorporation of market, industry, and firm-specific information into stock prices. *The Accounting Review* 79 (4): 1119–1151.
- Portes, R., and H. Rey. 2005. The determinants of cross-border equity flows. *Journal of International Economics* 65: 269–296.
- Robinson, L., and P. Stocken. 2013. Location of decision-rights within multinational firms. *Journal of Accounting Research* (forthcoming). Available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2294744##
- Roth, K., and S.W. O'Donnell. 1996. Foreign subsidiary compensation strategy: An agency theory perspective. *Academy of Management Journal* 39: 678–703.
- Schipper, K., and A. Smith. 1986. A comparison of equity carve-outs and seasoned equity offerings. *Journal of Financial Economics*: 153–186.
- Schoar, A. 2002. Effects of corporate diversification on productivity. *Journal of Finance*: 2379–2403.
- Shin, H. H., and R. Stulz. 1998. Are internal capital markets efficient? *Quarterly Journal of Economics* 113: 531–552.
- Shroff, N., A. Sun, H. White, and W. Zhang. 2013. Voluntary disclosure and information asymmetry: Evidence from the 2005 securities offering reform. *Journal of Accounting Research* 51 (5): 1299–1345.

Sidak, J. G. 2003. The failure of good intentions: The WorldCom fraud and the collapse of American telecommunications after deregulation. *Yale Journal on Regulation* 20: 207–67.

Siegel, J., and B. Z. Larson. 2009. Labor market institutions and global strategic adaptation: Evidence from Lincoln Electric. *Management Science* 55 (9): 1527–1546.

Smith, P. 2001. How do foreign patent rights affect U.S. exports, affiliate sales and licenses? *Journal of International Economics* 55: 411–439.

Stein, J. 1997. Internal capital markets and the competition for corporate resources. *Journal of Finance* 52: 111–133.

Wahid, A., and G. Yu. 2013. *Accounting Standards and International Portfolio Holdings*. Working paper. Available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1430589

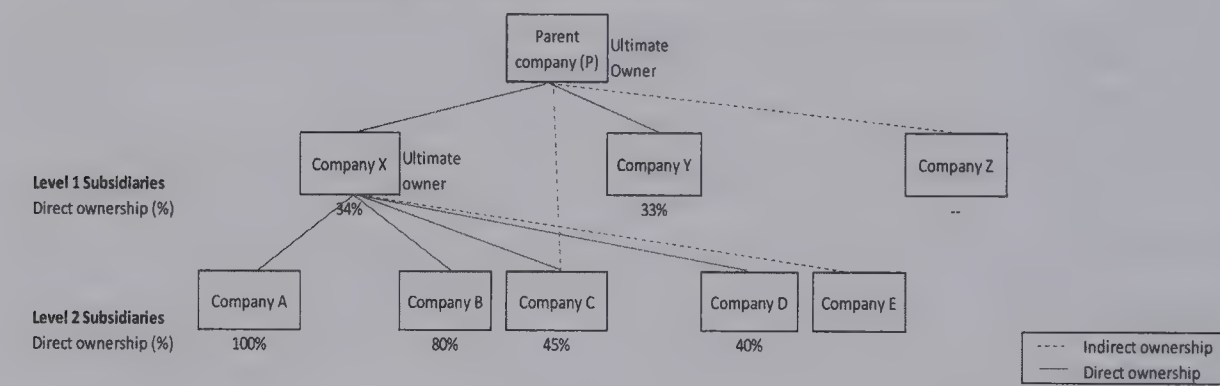
Wurgler, J. 2000. Financial markets and the allocation of capital. *Journal of Financial Economics* 58: 187–214.

Zaheer, S. 1995. Overcoming the liabilities of foreignness. *Academy of Management Journal* 38: 341–363.

Zhang, X. 2006. Information uncertainty and stock returns. *Journal of Finance* 61: 105–136.

APPENDIX A

Linking Parent and Subsidiaries in ORBIS Database



Parent-subsidiary dataset structure			
Parent	Subsidiary	Levels	Ownership (%)
P	X	1	34%
P	Y	1	33%
P	A	2	34% (=34%*100%)
P	B	2	27% (=34%*80%)
P	C	2	15% (=34%*45%)
P	D	2	14% (=34%*40%)

Step 1: Identify parent companies from the ultimate owner¹ dataset in ORBIS.

Step 2: Link subsidiaries (Level 1) to the parent companies. We define subsidiaries as firms that are held with more than 25% direct ownership.² We exclude firms that are indirectly held.³

Step 3: Link subsidiaries (Level 2) to the Level 1 subsidiary companies. Repeat process for Level 3 and 4 subsidiaries.

- 1) Ultimate owner: A firm where the shareholder with the highest direct ownership is an independent entity. To be an independent entity, the shareholder must be an individual or an entity with no shareholder owning more than 25 percent ownership.
- 2) Direct ownership: ownership representing voting shares that are directly held.
- 3) Indirect ownership: ownership representing voting shares that are held through an unknown entity.

APPENDIX B

Additional Tests_Untabluated: <http://dx.doi.org/10.2308/accr-50463.s1>

BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Business, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

RALF EWERT and ALFRED WAGENHOFER, *Earnings Management, Conservatism, and Earnings Quality*, Foundations and Trends® in Accounting (Hanover, MA: now Publishers Inc., 2012, ISBN 978-1-60198-602-3, Vol. 6, No. 2, pp. x, 123).

Ewert and Wagenhofer's (hereafter, EW) monograph presents information economics-based research on earnings management, earnings quality, and conservatism. The monograph focuses on rational expectations models, in which stock price is a key factor in the manager's reporting decisions. EW largely ignore the related literatures on real effects and contracting. Notwithstanding the exclusion of these other perspectives, the authors are left with a large body of research to present and discuss.

The monograph would make an excellent reading for a Ph.D. class, particularly when supplemented by other readings on contracting and/or real effects that discuss the same topics. (EW recommend Kanodia [2007].) The EW monograph not only presents a large body of important research, it also conveys a sense of how to conduct impactful research. The authors demonstrate an appreciation for the views of a wide variety of accounting researchers (including researchers who might identify themselves as theorists or empiricists) and accounting standard setters and regulators. The connections EW make between these views are particularly insightful.

EW use the Fischer and Verrecchia (2000) model of earnings management as their starting point, focusing on linear equilibria, although noting that nonlinear equilibria have been used to explain observed patterns of earnings management (e.g., Guttman, Kadan, and Kandel 2006). Earnings management is personally costly to the manager. When that cost is common knowledge, the market perfectly undoes the manager's earnings management in the linear equilibrium. However, earnings management cannot be avoided—the manager engages in costly manipulation to credibly communicate true earnings. Under a nonlinear equilibrium, pooling is possible (two or more different true earnings amounts are reported as the same), preventing the market from perfectly undoing the earnings management. When the manager's cost of earnings management varies across managerial types and the manager alone knows his type, the market can no longer perfectly undo earnings management, even under a linear equilibrium.

EW present many variations of the basic linear equilibrium model, including their own model of accounting and real earnings management (Ewert and Wagenhofer 2005). In their model, earnings management is personally costly to the manager, while real management is costly to the firm's shareholders. Tightening accounting standards can lead to less accounting earnings management, as one might expect, but can also lead to increased earnings management. Key determinants are the exogenous observable compensation weights the manager's compensation places on first- versus second-period earnings and the manager's weight on first-period stock price, which is not observed by shareholders. It is when these exogenous parameters foster relatively limited accounting earnings management that tightening accounting standards (increasing the manager's cost of earnings management) can result in increased accounting earnings management. An important idea is that tightening accounting standards increases the earnings response

coefficient, which always increases the manager's incentive to engage in real earnings management but has an ambiguous effect on accounting earnings management.

EW move on to discuss models in which the precision of accounting measurements is uncertain, which can help explain observed features of earnings management. In these models, earnings management is motivated by the desire to both mislead investors into thinking earnings are better than they are and the inference investors will draw about earnings precision from the report (e.g., Kirschenheiter and Melumad 2002). When compared to similar research in finance or economics on other topics, the paucity of research that formally tests predictions from models is a striking absence from the accounting literature on earnings management. Li (2010), which builds on the work of Kirschenheiter and Melumad (2002), is an exception.

The monograph then discusses potential benefits to earnings management that can arise when earnings management is used to convey other private information managers have. A related contracting story that I particularly like is Demski (1998), in which earnings smoothing is used to convey a manager's acquired expertise. Before concluding the section on earnings management, EW discuss models that relax the assumption of investor rationality.

The institutional justification for some of the assumptions employed in this broad class of earnings management models is unclear to me. For example, what is the justification for the particular information asymmetries assumed about compensation and/or the manager's shareholdings? Also, in the base model, the manager's role is limited to reporting. Arguably, the stewardship role of accounting is absent. (In fairness, even contracting models in which stewardship is central express a narrow view of stewardship when compared to Ijiri [1975].) Moreover, the distinction between disclosure and recognition is limited. In earlier models of disclosure, managers were not allowed to lie. In the later but closely related models of recognition, the main modeling difference seems to be that managers can lie at a personal cost. From our experience with employee stock option accounting, is the main difference between disclosure and recognition that disclosed amounts are more reliable?

EW's inclusion of real earnings management and accrual reversals is a welcome dose of stewardship and accounting structure. However, I was left wondering if real and accounting earnings management are modeled as being too similar. Both types of earnings management reverse. Accounting earnings management reverses because of accrual reversals. Real earnings management reverses because manipulation to increase first-period earnings (e.g., skimping on R&D) is met with a more-than-offsetting decrease in second-period performance. In the model, the main difference is that the cost of earnings management is borne by the manager, while the cost of real management is borne by the firm's shareholders. In practice, are CEOs the ones who bear the costs of accounting earnings management? When earnings management is uncovered and penalized by the SEC or in private litigation, it seems instead to be the current shareholders who end up paying the vast majority of the fines and other costs.

Before concluding their discussion on earnings management, EW provide a brief discussion of the related contracting literature. Since this section discusses Arya, Glover, and Sunder (1998), let me note a couple of minor points of disagreement. First, bankruptcy constraints on payments can be thought of as a restriction on the outcome set, which is allowed for by the Revelation Principle's assumptions (as long as the constraints do not depend on the agents' private information). Second, our paper is only partly a literature review, using the Revelation Principle in a way other than it was intended (as a solution technique) by placing the principal (rather than a disinterested mediator) in the position of mechanism designer and using the Revelation Principle's assumptions to classify models of earnings management. We also present two new models. In one, allowing for earnings management can reduce the cost of hiring a manager by relaxing his individual rationality constraint, because he faces less short-run scrutiny. Would you want to live in a glass house (unless it is built by Mies van der Rohe or Philip Johnson)? While giving the manager short-term cover, the accounting structure (accrual reversals) preserves information about long-run performance. In the second model, allowing for earnings management can provide better managerial incentives, because it limits the principal's (e.g., the board's) role in bailing out the manager if things go awry. Installing an accounting system that allows for short-term earnings management serves as a commitment device by the principal not to intervene *ex post*. The lack of intervention is anticipated by the manager and motivates him to do a better job himself.

The second part of EW's monograph begins with a discussion of the standard setters' conceptual frameworks, noting that the IASB's interpretation of relevance is so broad that it is not useful in guiding standard setting. The section focuses on EW's recent research on earnings quality. The main point seems to be

“that the information content of reported earnings cannot be derived from simply observing the mechanics of an accounting standard, but it depends on subtle interactions between accounting standards, earnings management, and rational inferences of investors” (p. 50). EW define earnings quality as a reduction in the market’s uncertainty about cash flows due to reported earnings and then study how changes in other characteristics of the environment affect earnings quality, producing a mix of relatively straightforward and subtler results. The manager is assumed to value market prices, reported earnings, and smooth earnings and to find earnings management personally costly. Instead of describing such models as non-contracting models, one might describe them as reduced-form contracting models. Their model, which is also replete with accounting structure such as working capital accruals, is particularly useful in thinking through various definitions of earnings quality adopted in empirical studies (Subsections 3.5 and 3.6), although, as EW note, a disadvantage associated with any particular formalization is that “it is difficult to draw more general conclusions for variations in the model setting” (p. 73).

In the last section of their monograph, the authors discuss recent research on accounting conservatism, including their own. EW distinguish between two approaches to modeling conservatism: (1) conservatism as a change in probability distributions, or bias, which has been widely adopted in many models, and (2) conservatism as a restricted message space. EW discuss implied P-E relationships, the contracting and non-contracting usefulness of conservatism, and interactions between conservatism and earnings management.

Kwon, Newman, and Suh (2001) is an early managerial contracting model of conservatism. They show that, with a risk-neutral manager, conservatism reduces the expected cost of providing incentives, because the likelihood ratios are improved (the probability of obtaining a good outcome is reduced under both high and low effort). Glover and Lin (2013) show that, once accounting reversals are taken into account, the incentive effects of conservatism are neutralized unless the contracting relationship facilitates an “incentive spillback”—in that case, conservatism is again optimal. Gigler, Kanodia, Sapra, and Venugopalan (2009) study debt contracting and show that conservatism either does not matter or produces inefficiencies, because a conservative bias makes low reports less informative, challenging the popular view that conservatism protects debt-holders.

EW note that practices conventionally described as examples of accounting conservatism such as asset write-downs are inconsistent with the definition of accounting conservatism as bias. The recent paper of Beyer (2012) is an important exception to the bias formulation of conservatism, using practice as the starting point for modeling conservatism. Gao (2013) is also noteworthy in its two-step representation of accounting measurement, which seems more descriptive of practice than the formulations employed in other models of conservatism. He develops the standard argument that conservatism arises in response to managerial opportunism.

It seems to me that accounting conservatism is inherently a multi-period concept. We “don’t count our chickens until they’ve hatched” (delayed recognition of good news in order to subject it to additional verification) and are quick to diagnose and report anticipated failures and the reason for the failure (early recognition of bad news accompanied by expanded disclosure). In ongoing work with coauthors, we show that such a definition of accounting conservatism can facilitate efficient liquidation and other decisions (for example, those normally built into or triggered by debt covenant violations), because of the early diagnostic information that conservatism provides about potential bad outcomes. At the same time, conservatism provides efficient stewardship incentives, because the slow recognition of good news facilitates an incentive spillback from the second period to the first. In a capital market setting (which is outside the scope of our work), one can imagine the delay of good news preventing an over-heated response that might provide managers and others with incentives to “take the money and run.”

REFERENCES

- Arya, A., J. Glover, and S. Sunder. 1998. Earnings management and the revelation principle. *Review of Accounting Studies* 3 (1): 7–34.
- Beyer, A. 2012. *Conservatism and Aggregation: The Effect on Cost of Equity Capital and the Efficiency of Debt Contracts*. Working Paper 120, Rock Center for Corporate Governance at Stanford University.
- Demski, J. 1998. Performance measure manipulation. *Contemporary Accounting Research* 15 (3): 261–285.

- Ewert, R., and A. Wagenhofer. 2005. Economic effects of tightening accounting standards to restrict earnings management. *The Accounting Review* 80 (4): 1101–1124.
- Fischer, P. E., and R. E. Verrecchia. 2000. Reporting bias. *The Accounting Review* 75 (2): 229–245.
- Gao, P. 2013. A measurement approach to conservatism and earnings management. *Journal of Accounting and Economics* 55 (2-3): 251–268.
- Gigler, F., C. Kanodia, H. Sapra, and R. Venugopalan. 2009. Accounting conservatism and the efficiency of debt contracts. *Journal of Accounting Research* 47 (3): 767–797.
- Glover, J., and H. Lin. 2013. *Accounting Conservatism and Incentives: Intertemporal Considerations*. Working paper, Carnegie Mellon University.
- Guttman, I., O. Kadan, and E. Kandel. 2006. A rational expectations theory of kinks in financial reporting. *The Accounting Review* 81 (4): 811–848.
- Ijiri, Y. 1975. *Theory of Accounting Measurement*. Volume 10. Sarasota, FL: American Accounting Association.
- Kanodia, C. 2007. *Accounting Disclosure and Real Effects*. Hanover, MA: now Publishers Inc.
- Kirschenheiter, M., and N. D. Melumad. 2002. Can “big bath” and earnings smoothing co-exist as equilibrium financial reporting strategies? *Journal of Accounting Research* 40 (3): 761–796.
- Kwon, Y. K., D. P. Newman, and Y. S. Suh. 2001. The demand for accounting conservatism for management control. *Review of Accounting Studies* 6 (1): 29–52.
- Li, W. 2010. Discontinuity in earnings distributions: A theory and evidence. Available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1296803

JONATHAN GLOVER
Professor of Accounting
Carnegie Mellon University

RICHARD K. FLEISCHMAN, WARWICK FUNNELL, and STEPHEN P. WALKER,
Critical Histories of Accounting: Sinister Inscriptions in the Modern Era (New York, NY: Routledge, 2013, ISBN 978-0-415-8870-3, pp. vi, 274).

Fleischman, Funnell, and Walker’s new book, *Critical Histories of Accounting: Sinister Inscriptions in the Modern Era*, is a welcome addition to the “Routledge New Works in Accounting History” series. The book opens with Funnell and Walker’s concise and helpful introduction that situates the volume’s papers within the wider accounting history and social science literatures. The authors also outline the four themes—annihilation, subjugation, exploitation, and exclusion—used to organize “eleven previously published, significant articles, authored by prominent critical accounting historians” (Introduction, p. 1). In outlining these themes, the introduction also draws out commonalities across the papers, commonalities that serve to explain the significance of the book’s subtitle. Although drawing upon diverse historical episodes that span over two centuries and across several continents, each paper illustrates how a particular group used accounting practices (or professional accounting associations) to help accomplish objectives or plans that disadvantaged, exploited, subjugated or even killed others. These illustrations are further elaborated by Fleischman (Chapter 1) in a previously unpublished chapter. This chapter provides an overview of some theoretical debates that have occurred within the critical accounting history literature and offers a glimpse of the wide-ranging set of issues that have been addressed by critical accountants. The Introduction and first chapter successfully establish the aims of the volume as well as provide an entrée into the critical accounting literature for those who wish to explore it further.

Annihilation

Funnell and Walker describe the volume’s first theme as consisting of efforts to exterminate another group so thoroughly as to leave no trace. Given its frequent characterization as a neutral, calculative practice, accounting seems unlikely to be associated with such efforts. However, two chapters illustrate the possibly

“essential contributions of accounting to governments and individuals who are determined to carry out policies of annihilation” (Introduction, 2). Funnell (Chapter 2) investigates the role of accounting in the bureaucratic apparatus established to enact the Nazi Final solution. Here, accounting served the important function of allowing administrators and bureaucrats to separate themselves from the consequences of their work by reducing individual, unique humans to numbers—the number killed or remaining to be killed, the transportation bills for shipments of persons to concentration camps, etc. Importantly, accounting reports were also employed as a technique to maintain the accountability of the SS personnel who sorted property confiscated from exterminated Jews. Funnell argues that the detailed inventories of such confiscated assets were seen to sanctify this activity by demonstrating that the confiscation was not motivated by “selfish greed but by the need to purify the German race” (p. 67).

Lippman and Wilson (Chapter 3) also draw upon accounting practices used during the Holocaust to support their argument that the choices of accountants about what information to present as well as its form of presentation has important effects upon the decisions made by organizations and other entities. They demonstrate how reports presenting the revenues and expenses associated with the use of slave labor served to depict such slaves as productive inputs that could be used for the financial advantage of private German firms. Such reports failed to recognize the horrifying conditions under which the Jewish slaves labored even as the reports employed estimates of the useful life of slave laborers. As another example, the authors describe the cost/benefit analysis employed to reach the “rational” decision to burn children alive rather than to incur the additional costs of gassing them prior to incineration.

Subjugation

Three chapters explore the volume’s second theme. In contrast to policies of annihilation, policies of subjugation seek to subdue a population and often involve the efforts of an imperial power to gain access to, and control over, the property of others. Funnell and Walker (Introduction, 4) describe these chapters as showing the uses of accounting information to define the issues and problems associated with consolidating and extending imperial power as well as to “create political and social discourses . . . which would then inform and legitimize political debate and strategies essential to the preferred imperial political goals and beliefs.”

The section opens with Graham and Neu’s (Chapter 4, p. 85) study of the “mundane technologies of accounting” employed by the Canadian government in its efforts to convert the indigenous population to economic citizenship. The authors describe the different methods employed by local agents to convert observations about indigenous life into the reports submitted to government centers. Such reports were then used by politicians and others to assess the accomplishment of control over indigenous populations as well as the need for further actions. The policies enacted by the Canadian government were underpinned by a paternalism that presumed First Nation’s peoples should be treated as “wards or children of the state” (p. 91) and stripped of their agency or subjugated. Graham and Neu show how the administrative reports of the era reflected back these beliefs by representing this governmentally imposed lack of agency as evidence of the continuing need for paternalism.

Walker (Chapter 5) explores the social implications of accounting publicity in the administration of British poor laws. Although he notes that systematic accounting was used as more commonly thought—to control expenditures and prevent mismanagement of public funds—he also stresses its importance for controlling the pauper. Printed lists of paupers provided various details about such persons, including the ascribed reasons for relief (e.g., aged, bastard, cripple). In so doing, the lists often placed “an additional stigmatizing label on the recipient of relief” (p. 105). The lists also permitted surveillance of the poor by facilitating the capacity of community members to identify the “undeserving poor.” This identification of paupers also worked to distance the poor from their neighbors. The potentially stigmatizing effects of ascriptions, coupled with this distancing from neighbors, served a further purpose in discouraging some potential claimants from applying for aid and thereby reducing public expenditures for the poor.

O’Regan (Chapter 6) illustrates how British government responses to the Irish Famine of the mid-1840s relied upon a “vast architecture of accounting.” This architecture included detailed lists to identify those capable and “deserving” of work; weekly reports were sent to Dublin, detailing the name of laborers, nature and quantity of work performed, and rate of payment (p. 118), and various accounting and internal control practices were introduced to ensure that payment was made only for work performed. O’Regan argues that these controls, reports, and numbers were employed to reform the perceived moral lapses of the Irish by replacing “indolence with industry, passivity with endeavour” (p. 116) as well as to discipline the behavior of those authorized to provide relief.

Exploitation

Three chapters explore the connections between accounting and exploitation. Funnell and Walker (Introduction, pp. 6–7) describe exploitation as a moral concept, one that denotes taking advantage of another person in a way that degrades her/him. Hopper (Chapter 7) focuses on the exploitation of labor and outlines how four epochs of capitalism have shaped, and have been shaped by, management accounting and efforts to control labor processes. During the first epoch, detailed records of wages and the introduction of new systems for determining prime cost marked “the beginning of a transfer of financial knowledge from the worker to the factory owner . . . [which] helped make visible where returns might best be creamed off and labour be intensified and disciplined” (132). In the second epoch, Taylorism and the development of standard costs increased control over labor processes, as product jobs were redesigned and simplified to allow for the replacement of craftsmen with semi-skilled labor. In the third epoch, Fordism, new accounting techniques such as transfer pricing, flexible budgeting, and ROI measures accompanied the exploitation of economies of scale through increased firm concentration and vertical integration. The fourth epoch saw decreases in manufacturing jobs and union membership and rising numbers of low-wage workers, as flexible manufacturing, subcontracting of low-value-added activities, and other practices came to characterize workplaces.

Oldroyd, Fleischman, and Tyson (Chapter 8) explore the role of accounting in the U.S. and in the British West Indies (BWI) slave economies. They maintain that their work shows accounting as “an instrument of both social control and social change” (p. 155). The paper argues that accounting helped to perpetuate the myth that slaves were less than human by classifying slaves as inventory alongside livestock such as cattle and mules. The authors also contend that accounting may have served a more protective role by highlighting the value of slaves and thereby contributing to incentives to maintain their health and well-being. For me, this argument falls flat, as similar statements might be made to argue that accounting contributed to the better treatment of other property like cattle. So, while accounting may have provided plantation agents with incentives to feed and house slaves sufficiently to ensure their continuing capacity to work, accounting would not have given incentives to provide slaves with the conditions to prosper as human beings (access to freedom, justice, etc.). However, the authors’ illustrations of accounting’s use as an emancipative force in the BWI provides clearer indications of the positive potentialities for accounting.

Hooper and Kearins (Chapter 9) explore another case in which accounting and other expertises assisted in implementing policies to exploit and disadvantage an indigenous group. They document how such expertises were used to exploit the Maori of New Zealand and gain control over Maori lands while allowing politicians to “maintain a façade of sympathy” (p. 160) toward the Maori. Accounting facilitated this hypocrisy by distancing the actions associated with dispossession from the realm of active political decision-making. Dispossession was accomplished through the activities of experts who established the rents received and commissions charged for Maori lands held in public trust as well as determined the balances paid to, or withheld from, the Maori. Accounting statements of income and distributions captured the results of this activity by experts and subsequently were used to show that the lands held by the Maori generated low returns. This “evidence” was then used to justify the sale of these lands. The authors argue that “while accounts may convey a sense of facticity and neutrality, both these attributes are suspect. . . . Accounting acts as a key technology in achieving a redistribution of assets, particularly where accounts can be shown to reveal a poor return” (p. 167).

Exclusion

In this final section, attention shifts away from accounting practices, reports, and techniques and toward the ways that professional accounting groups have employed practices of exclusion. Three chapters examine the various devices used to exclude groups such as women, African-Americans, and Afro-Caribbeans from membership in professional groups and from the practice of accounting. In their contribution to this section, Kirkham and Loft (Chapter 10) argue that the exclusion of women was fundamental to the professionalization of accounting in England and Wales in the period 1871–1931. The authors document a significant increase in the numbers of women employed as clerks and bookkeepers during this period even as women were excluded from professional accounting groups. They then convincingly argue that these parallel developments were associated with a discursive construction of bookkeeping as feminine and of accounting as masculine, a construction that facilitated the exclusion of women from the ranks of association members even as it served to establish accounting as a profession.

Exploring the experiences of African-Americans attempting to gain entry into the U.S. profession during the years 1914–1965, Hammond and Streeter (Chapter 11) provide another vivid account of the exclusionary

practices of the accounting profession. In order to qualify for the CPA exam (and licensure), most states required the completion of a one- to three-year apprenticeship with a practicing CPA. This requirement posed a significant obstacle for otherwise qualified African-Americans, as their employment opportunities were negligible at white firms. Indeed, by 1945, only nine African-Americans had earned their CPA designation. The authors focus on the experiences of individual African-Americans in order to illustrate the combination of “unique characteristics and individual effort” (p. 194) that characterized the success of the earliest African-American CPAs. Interview excerpts provide us with an opportunity to listen to these individuals describe their experiences and to provide a sense of the obstacles they overcame. For example, Bernadine Gines poignantly recounts her experience in searching for employment after completing an M.B.A. at New York University. During her first interview, she described how “One of the partners advised that he would not give me a job, but that his wife was looking for a maid if I knew anyone who wanted that job” (p. 195). Despite this unpromising beginning, Ms. Gines persisted, found employment, and eventually earned her CPA.

Annisette (Chapter 12, p. 200) explores “the multiple ways in which race has been deployed to bring about majority under-representation and exclusion” in Trinidad and Tobago (T&T). In colonial T&T, accountancy was associated with white “Britishness.” Indeed in 1938, all six professionally qualified accountants were British nationals. With the expansion of the corporate sector during the colonial period, the race-based division of labor was further refined with the importation of British accountants to perform the more “expert” work, with the Coloured/Mixed category performing bookkeeping tasks and with African and East Indian segments of the T&T population excluded from accounting. British professional societies such as the ICAEW and ACCA contributed to this situation with requirements such as the completion of apprenticeships in Britain or the completion of “approved accountancy work.” These requirements effectively barred Afro- and Indo-Trinidadians from the accounting profession, given their limited opportunities for obtaining such employment. Following independence in 1962, the newly formed T&T professional association was eventually dominated by white chartered, practicing firms. Annisette details how various policies enacted by this association effectively continued earlier practices of racial exclusion, albeit using more subtle mechanisms. She argues that these actions illustrate how the “establishment of alternative routes to the profession can facilitate new patterns of exclusion as more accessible routes themselves become racialized” (p. 214).

Although editors can always be taken to task for selecting article x rather than article y for inclusion in an edited volume, the reviewer’s task is to evaluate whether the collection as a whole works to accomplish the volume’s aims. Although the brief summaries provided here do not capture the complexity and nuances found in these articles, the summaries do provide ample evidence that these articles effectively elucidate the organizing themes of annihilation, subjugation, exploitation, and exclusion. Furthermore, the collection seems likely to achieve the purpose stated in the Introduction—an effort to interest a new generation of scholars in the “intellectual possibilities and rewards of doing research in the area” (Introduction, 1). The chapters provide a solid overview of the wide-ranging sets of questions that may be, and have been, addressed within accounting history research. The chapters also illustrate the variety of materials that may be employed as evidence in developing answers to such questions. Finally, this rich variety of questions and materials coupled with the quality of writing across the chapters also suggests creative challenges and opportunities in conducting accounting history research. As such, the editors have succeeded in producing an introduction to the possibilities for and rewards of accounting history research.

Although introducing a new generation of scholars to such possibilities and rewards is an admirable aim, the contribution of the volume is not limited to this purpose and deserves a wider readership. The volume might also be usefully employed in accounting seminars at the master’s or perhaps advanced undergraduate levels. By providing diverse illustrations of the ways in which accounting practices and reports have been used to facilitate the imposition of harms upon others, the volume works to contest claims that accounting is a value-free technology that simply produces clear and accurate information. These illustrations offer many possibilities for engaging in lively and thought-provoking discussions on topics such as the “appropriate” uses of accounting, the “appropriateness” and limits of different conceptions of professional and individual responsibility, as well as the “appropriateness” of continuing to characterize accounting as a technical practice and the opportunity to formulate alternative characterizations.

JONI J. YOUNG
Professor of Accounting
University of New Mexico

DAVID HATHERLY, *The Failure and the Future of Accounting: Strategy, Stakeholders, and Business Value* (Farnham, Surrey, U.K.: Gower, 2013, ISBN 978-1-4094-5354-3, pp. xviii, 222).

Ex Caledonia semper aliquid novi (as Aristotle, or Pliny, or Erasmus might have said).¹ Many innovative and thoughtful proposals for accounting reform have emanated from Scotland. David Hatherly, professor emeritus at the University of Edinburgh, has written a book that provides a radical suggestion for change based on an overarching theory that seeks to define appropriate disclosures and accountability on the basis of different firms' business models. There have been a number of recent proposals that aspects of financial reporting should be framed on the basis of the reporting firms' business models and that commentary and discussion in narrative reports should discuss the business models of reporting firms (IASB 2009; FRC 2010; IIRC 2013). Hatherly provides a theory of business models that seeks to articulate what should be disclosed and discussed under different business models.

The work belongs to the long tradition of normative accounting theories such as Edwards, Kay, and Mayer (1987) and Edwards and Bell (1961), although it is grounded in the author's experience of the problems faced by real companies, rather than economic theories based on stylized assumptions. It is written for an audience that is not familiar with accounting, even in its conventional forms, and so it is designed as a primer for the uninitiated, rather than arguing in a scholarly fashion about the wide range of pre-existing theories.

The book has a main theme of coordinating strategy, stakeholder networks, and accounting together with a second theme of coordinating accounting with the matching of production and consumption in the macro-economy. At the heart of this ambitious program is "four slice" (4S) accounting based on analyzing a company's market capitalization into "four slices" of equity. Hatherly's analysis arises from his diagnosis of the role of accounting in the financial crisis and the belief that there must be a better way.

Chapters 1–3 set forth the author's view that traditional accounting (by which he means both external and internal accounting) has failed. Chapter 1 begins with an assumption that business is best understood through coordination of three representations of business: business as strategy, business as stakeholder network, and business as value. The approach is derived from the work of cognitive anthropologist Edwin Hutchins, which emphasizes the distributed nature of cognition among individuals as part of a group.

The framework that Hatherly sets up is complex and multi-dimensional:

Equity is divided into four slices (4S): traditional tangible assets and working capital; intangibles; promises; prospects.

The business model is characterized by the mnemonic "LEADERS" (Leverage, Engage, Align, Develop, Evaluate, and ReShape).

Stakeholders comprise: customers, suppliers, employees, investors, government, management, and the company itself.

Selection engagement and alignment with stakeholders depends on six dimensions of the business: product specification, price and volume, stakeholder knowledge and capabilities, relationship with the stakeholder contractual promises to the stakeholder, and prospects for the entity and its business model.

The framework therefore seems to result in a four-dimensional matrix of 1,008 cells ($4 \times 6 \times 7 \times 6$). This is quite a lot to buy into, and the book's subsequent development attempts to persuade the reader of the validity of this meta-model as a basis for internal and external accounting. The author also assumes that the new economy of knowledge and stakeholder networks is more beneficial than the old economy based on traditional asset-liability structures. This reviewer tends to be skeptical of such claims, because there were so many of them preceding the crashes of 1999–2000 and 2007–2008.

Resulting from the framework is a division of profits into productive profit and speculative profit. Productive (or phase one) profit is earned on outputs for current consumption. Three kinds of speculative profit are identified: phase two profit arises from securitization of phase one profits, phase three profit arises from changes to the capital value of phase two profits, and phase four profits are fees and commissions arising from "initial capitalisation or with the management, trading or repackaging of existing capital" (p. 6).

¹ There is always something new out of Caledonia.

The author suggests that, when dividends, managerial bonuses, or taxation arising from speculative profits are spent on consumption, there is a mismatch between consumption and production in the economy. Lending by banks to finance consumption not matched by production gives rise to phase five profits. He further suggests that mismatch between production and consumption is a fundamental economic flaw and that accounting has an important role in remedying it. More generally, Hatherly suggests that accounting failure was a cause of the 2008 financial crisis, allowing the financial sector to report conversion of capital into consumption as good news contributing to unproductive “financialisation” of the economy.

The remedy, 4S accounting, is characterized as a kind of separate fund accounting for each of the slices of equity. In particular, acquisitions are accounted for by dividing the purchase price among the four funds of the acquired company and proportionately consolidating them with the funds of the acquirer; hence, there is no minority interest. No goodwill arises and, hence, there is no need for impairment testing.

Chapter 2 presents an outline of traditional accounting statements by means of an example, together with a critique of performance measures. Hatherly charges traditional accounting with overstating return on equity because of unrecorded intangibles, and of preferring service industry companies to businesses requiring large investment in fixed assets, because of the higher return on capital employed that is required. This argument is unclear: it has long been known that the NPV criterion is superior to profitability measures, and the most basic of business education now includes this. Hatherly’s assertion that traditional accounting causes a systematic reallocation of manufacturing to economies that prefer maximizing productive assets from ones that prefer maximizing return on capital needs considerable development before it could be considered a persuasive reason to adopt his 4S accounting. At the same time, the standard-setting bodies’ own standards suffer from a lack of articulation about the behavioral assumptions underpinning their work, and if they were made explicit, they might look quite similar. Hatherly’s work deserves a mighty cheer for stating explicitly that financial reporting has a macro-economic role as well as a micro-role. If the FASB and the IASB acknowledged that an underlying purpose of financial reporting was resource allocation within the economy, then the claims they make could be subject to more rigorous scrutiny than is currently the case.

Chapter 3 seeks to demonstrate the failure of traditional accounting through an analysis of the Enron case. The lessons drawn are that information about Enron’s performance (feedback) was lacking because of a neglect to focus on intangibles, failing to relate risk and performance, no separate reporting of legacy promises, and not separating past and future performance. Many different stories have sought to explain the Enron debacle; nevertheless, Hatherly provides a stimulating version of events.

The nature of the stakeholder knowledge network is elaborated in Chapter 4. This is not the ethical theory of the organization propounded by Freeman and by Donaldson; it is the view, derived ultimately from March and Simon perhaps, in which the organization is considered as a nexus of capabilities co-operating, but at the same time competing for rents. Organizations earn surpluses (create value) by leveraging the knowledge-based capabilities of their stakeholders. Hatherly provides a schema for managing stakeholder relationships that he analyzes by each stakeholder group.

The accounting model derived from this framework appears in Chapter 5. Using a residual income approach, a firm’s market capitalization is divided into the value (ESTA) of assets in place—traditional assets and liabilities plus existing intangibles—and growth potential. An estimated market rate of return is used to estimate profit derived from the traditional net assets measured at book value. When deducted from an adjusted total profit, the residual income from existing intangibles is derived and used to capitalize, at a differently risk-adjusted cost of capital, the current use value of intangibles. The difference between the sum of these two items and actual market capitalization is characterized as the value of expected changes in intangibles (growth and opportunity value, GOV). An illustrative example based on the pub chain J D Wetherspoon plc is provided. But we are not finished. Finally, the net value of promises (NVP) to stakeholders to secure their continuing involvement, such as pension fund deficits and deferred taxation, are introduced. The 4S model thus becomes:

$$\text{Market Capitalization} = \text{ESTA} + \text{CUVI} - \text{NVP} + \text{GOV},$$

in which, $\text{ESTA} + \text{CUVI}$ makes up productive capital and $\text{GOV} - \text{NVP}$ is regarded as speculative capital. Rather as in the case of market value added calculations, adjustments are required to GAAP numbers to arrive at an estimate of sustainable profits.

Hatherly connects the model to Penman's (2007) reasoning about when fair value is the appropriate value: in the 4S model, ESTA and CUVI are based on inputs and outputs, so it is appropriate to use a cost based measurement, whereas NVP is a result of value changes and should be measured at fair value. As GOV is a residual involving MC and the other amounts, it is a mixed measurement.

Chapter 6 examines the nature of risk as it applies to the different slices of equity. As the cost of different kinds of capital is crucial to the model, some means of determining these costs is required. The book acknowledges the problem and offers the solution that management should disclose the cost of capital as it applies to the different slices; however, as a basis for financial reporting this would result in much discretion being placed in the hands of directors. The chapter goes on to a more general discussion of risk, taking up the theme of distributed risk and three views of business risk as strategic risk, value at risk, and stakeholder risk.

Chapter 7 is a discussion of accounting for intangibles, critiquing current accounting requirements, suggesting a treatment of intangibles in 4S accounting, and setting out two case studies. The subsequent chapter covers "promises," that is to say commitments to stakeholders that form part of the current "proposition" to engage their participation in current activities. These promises include customer warranties, defined benefit pension schemes, deferred tax, fixed interest loans, share options granted to management, and fixed asset replacement. Hatherly discusses setting aside liquid funds to meet such promises—as in a pension fund—but any unmatched promise is regarded as a liability. He observes that, while accounting for defined benefit pensions meets desirable standards, accounting for other promises, such as deferred tax, do not.

Chapter 9 takes up the connection of accounting with strategy, including case studies analyzed using the LEADERS approach applied to the stakeholder network. Financialization is defined as occurring when speculative capital grows faster than productive capital.

Chapter 10 demonstrates analysis of the process through use of 4S calculations. The chapter is the source of Hatherly's most far-reaching recommendation that only growth in productive capital should be eligible for distribution of dividend or payment of managerial bonuses. Growth in speculative capital should be eligible only for reinvestment in productive capital. In the case of banks, productive capital is defined as loans to support production, and banks would likewise be prohibited from making distributions out of speculative operations. The radical suggestion is put forward that banks should be permitted to distribute only profits arising from lending to businesses for productive investment, and that all other gains should be reinvested in productive investment. Financial reporting by banks would therefore need to identify the source of gains and, in the process, provide more useful information for their own governance and for the management of the economy. The chapter ends with the thought that "Bank regulation must be designed to incentivize lending to productive businesses. If it does not then it is by definition counterproductive" (p. 168).

Chapter 11 is a series of short discussions of current issues in financial reporting viewed from a 4S perspective. Chapter 12 provides a worked illustration of 4S Financial Statements, and Chapter 13 is a summary of the whole work.

Hatherly's 4S solution can be interpreted on a number of levels. It is a scheme for internal and strategic analysis within an organization, an alternative form of external financial analysis, and a basis for a radical overhaul of external financial reporting. Such schemes of analysis and reporting are not unique. They are best regarded as models, in the sense that a road map is a model, which accentuate features of interest and omits others. They are not representations of reality; so, for example, what 4S accounting presents as growth opportunities (GOV) is an approximation, where divergence from the underlying concept, which is unobservable, may or may not be important. Accounting prescriptions that treat the proxy as the thing are difficult to evaluate.

There are serious obstacles to 4S becoming a new basis for financial reporting. Some are inherent in the method, such as reliance on two costs of capital determined by management. Ultimately, the major obstacle is the same as the one that meant the superior Betamax standard lost out to VHS: there is probably too much investment and support for the old standard to overturn it.

Viewed as a tool for analysis of existing information, 4S is an extension of residual income and so has the need to make adjustments to conventional information of a more or less transparent nature. M.B.A. students conducting 4S analyses of organizations to which they had good access would have a very interesting project, and good ones would be able to evaluate not only the organizations' strategies and networks but also the strengths and shortcomings of the 4S approach. The book is interesting in its own right to scholars of financial reporting systems and should also be read by standard setters to remind them of the economic consequences of

their work. It also serves as a useful counterweight to alternative conceptualizations of the business model (e.g., IIRC 2013) and to remind us all that alternatives exist and choice among them should be evidence-based.

REFERENCES

- Edwards, E. O., and P. W. Bell. 1961. *The Theory and Measurement of Business Income*. Berkeley, CA: University of California Press.
- Edwards, J., K. Kay, and C. Mayer. 1987. *The Economic Analysis of Accounting Profitability*. Oxford, UK.: Oxford University Press.
- Financial Reporting Council. 2010. *The UK Corporate Governance Code*. London, U.K.: Financial Reporting Council.
- International Accounting Standards Board. 2009. *IFRS 9 Financial Instruments*. London, U.K.: IASB.
- International Integrated Reporting Council (IIRC). 2013. *Consultation Draft of the International <IR>Framework*. Available at: <http://www.theiirc.org/wp-content/uploads/Consultation-Draft/Consultation-Draft-of-the-InternationalIRFramework.pdf>
- Penman, S. H. 2007. Financial reporting quality: Is fair value a plus or a minus? *Accounting and Business Research, International Accounting Policy Forum* 37 (Supp 1): 33–44.

MICHAEL PAGE

Professor Emeritus of Accounting
Portsmouth Business School

Errata

The authors, Liesbeth Bruynseels and Eddy Cardinaels, regret an error in their article “The Audit Committee: Management Watchdog or Personal Friend of the CEO?” published in *The Accounting Review*, Vol. 89, No. 1, pp. 113–145. In Section III, “Data Description,” the statistics listed—describing the types of voluntary organizations that generate social ties between a CEO and audit committee members through “other activities” in which both participate—were erroneously generated based on “other activities” for all U.S.-listed firms’ CEOs and director profiles available in the BoardEx database for the period 1999–2009.

When the authors correctly restricted the sample to the other activity ties between the CEO and audit committee members for only the 11,963 firm-year observations included in the paper’s analyses, this generated changes in the originally reported results. On the third line from the bottom of page 119, the sentences beginning “In our sample, . . . “ and continuing through “. . . percent).⁵” should be replaced with the following text:

In our sample, audit committee members have other activity ties to the CEO through 571 distinct voluntary associations. Similar to Fracassi and Tate (2012, 162), we manually collected background information on the 39 voluntary associations that each account for at least five distinct CEO-audit committee member ties within our sample firm-year observations. This analysis shows that the majority of other activity ties between a CEO and audit committee members are formed through memberships in exclusive country clubs, charities, or leisure clubs (33.3 percent, 10.3 percent, and 7.7 percent, respectively). Other activity ties are also formed through industry associations or representation bodies with limited membership (30.8 percent) or through the networks of trustees, councils, or supervisors of either regional non-profit institutions (12.8 percent) or federal advisory and research institutes (5.1 percent).⁵

These sentences respond to a reviewer’s request to clarify the nature of ties between a CEO and audit committee members via their joint participation in “other activities.” As such, the errors in this supplementary disclosure do not affect any of the empirical results described in the text or reported in the tables of the manuscript.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* “should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research.”

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and editors are responsible for providing constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

Format

1. All manuscripts should be formatted in 12-point font on 8 ½ × 11" paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial “we”.
5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.
6. The American Accounting Association encourages use of gender-neutral language in its publications.
7. Experimental studies using human subjects should contain a footnote affirming that approval has been granted by the institution where the experiment took place.
8. Headings should be arranged so that major headings are centered, bold, and capitalized. Second-level headings should be flush left, bold, and both uppercase and lowercase. Third-level headings should be flush left, bold, italic, and both uppercase and lowercase. Fourth-level headings should be paragraph indent, bold, and lowercase. Level one headings should be numbered in Roman numerals. Subsequent subheadings should not be numbered. For example:

I. A CENTERED, BOLD, ALL CAPITALIZED, FIRST-LEVEL HEADING

A Flush Left, Bold, Uppercase and Lowercase, Second-Level Heading

A Flush Left, Bold, Italic, Uppercase and Lowercase, Third-Level Heading

A paragraph indent, bold, lowercase, fourth-level heading. Text starts . . .

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: three days; 3 kilometers; 30 years. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text; in tables and figures, the symbol % is used.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: a cross-sectional equation; re-form. See *Webster's* for correct usage.

Keywords: The abstract must be followed by at least three keywords to assist in indexing the paper and identifying qualified reviewers.

Abstract/Introduction

An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. The Keywords statement should appear immediately below the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

Tables and Figures

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.
6. When information is not available, use "NA" capitalized with no slash between.
7. Figures must be prepared in a form suitable for printing.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Within-text citations are made using an author-year format. Cited works must correspond to the list of works listed in the "References" section. Authors should make an effort to include the relevant page numbers in the within-text citations.

1. In the text, works are cited as follows: author's last name and year, without comma, in parentheses. For example: one author, (Berry 2003); two authors, (Fehr and Schmidt 2003); three to five authors, (Scholes, Wolfson, Erickson, Maydew, and Shevlin 2008); six or more authors, (Dikolli et al. 2013); more than one work cited, (Cole and Yakushiji 1984; Dechow, Sloan, and Sweeney 1995; Levitt 1998); with two works by the same author(s), (Nelson 2003, 2005).
2. For repeated citations of works that have three or more authors, use only the first author's last name followed by "et al." (et is not followed by a period): first citation, Dechow, Kothari, and Watts (1998); subsequent citations, Dechow et al. (1998).
3. Unless confusion would result, do not use "p." or "pp." before page numbers. For example, (Dechow and Dichev 2002, 41–42).

4. When the reference list contains two or more works by the same author (the only author or first of two or more authors) published in the same year, the suffix a, b, etc., is appended to the date in the within-text citations and in the “References” section. For example, (Johansson 2004a, 2004b, 2004c; Baiman and Rajan 2002a, 2002b; Dhaliwal, Erickson, and Li 2005a; Dhaliwal, Krull, Li, and Moser 2005b).
5. When the author’s name is mentioned in the text, it need not be repeated in the citation. For example: “Cohen et al. (2005) provide . . .”
6. Citations to institutional works should use acronyms or short titles where practicable. For example: (NCFRR, The Treadway Commission 1987).
7. If the paper refers to statutes, legal treatises, or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a “References” section that contains only those works cited within the text. Each entry should contain all information necessary for unambiguous identification of the published work. Use the following formats (which follow *The Chicago Manual of Style*):

1. Arrange citations in alphabetical order according to the surname of the first author or the name of the institution or body responsible for the published work.
2. Use authors’ initials instead of proper names.
3. For two or more authors, separate authors with a comma, including a comma before “and” (Dechow, P. M., R. Sloan, and A. Sweeney).
4. Date of publication follows the author’s (authors’) name(s).
5. Titles of journals or newspapers should not be abbreviated.

Sample entries are as follows:

- Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.
- Baiman, S., and M. Rajan. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.
- Berry, R. 2003. Testimony before the Senate Committee on Homeland Security and Governmental Affairs Permanent Subcommittee on Investigations. November 18. Available at: <http://hsgac.senate.gov/files/111803berry.pdf>.
- Cohen, D., A. Dey, and T. Lys. 2005. *The Sarbanes Oxley Act of 2002: Implications for Compensation Structure and Risk-Taking Incentives of CEOs*. Working paper, New York University, University of Chicago, and Northwestern University.
- Cole, R., and T. Yakushiji, eds. 1984. *The American and Japanese Auto Industries in Transition*. Ann Arbor, MI: University of Michigan.
- Dechow, P. M., R. Sloan, and A. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.
- Dechow, P. M., S. P. Kothari, and R. L. Watts. 1998. The relation between earnings and cash flows. *Journal of Accounting and Economics* 25: 133–168.
- Dechow, P. M., and I. Dichev. 2002. The quality of accruals and earnings: The role of accrual estimation errors. *The Accounting Review* 77 (Supplement): 35–59.
- Dhaliwal, D., Erickson, and O. Li. 2005a. Shareholder income taxes and the relation between earnings and returns. *Contemporary Accounting Research* 22: 587–616.
- Dhaliwal, D., L. Krull, O. Li, and W. Moser. 2005b. Dividend taxes and implied cost of equity capital. *Journal of Accounting Research* 43: 675–708.
- Dikolli, S. S., J. H. Evans III, J. Hales, M. Matejka, D. V. Moser, and M. G. Williamson. 2013. Testing analytical models using archival or experimental methods. *Accounting Horizons* 27 (1): 129–139.
- Easton, P. 2003. Discussion of: The predictive value of expenses excluded from pro forma earnings. *Review of Accounting Studies* 8: 175–183.
- Engel, E., R. Hayes, and X. Wang. 2007. The Sarbanes-Oxley Act and firms’ going-private decisions. *Journal of Accounting and Economics* (forthcoming).
- Fehr, E., and K. Schmidt. 2003. A theory of fairness, competition, and cooperation. In *Advances in Behavioral Economics*, edited by C. Camerer, G. Loewenstein, and M. Rabin, 271–296. New York, NY: Princeton University Press.
- Financial Accounting Standards Board (FASB). 2006. *Accounting for Uncertainty in Income Taxes, and Interpretation of FASB Statement No. 109*. FASB Interpretation No. 48. Financial Accounting Series. Norwalk, CT: FASB.

- Janis, I. L. 1982. *Groupthink: Psychological Studies of Policy Decisions and Fiascos*. Boston, MA: Houghton Mifflin.
- Levitt, A. 1998. The numbers game. Speech delivered at New York University, Center for Law and Business, September 28.
- Maggi, G. 1999. The value of commitment with imperfect observability and private information. *RAND Journal of Economics* (Winter) 30: 555–574.
- National Commission on Fraudulent Reporting (the Treadway Commission). 1987. *Report of the National Commission on Fraudulent Financial Reporting*. Washington, D.C.: NCFRR.
- Nelson, M. W. 2003. Behavioral evidence on the effects of principles- and rules-based standards. *Accounting Horizons* 17 (1): 91–104.
- Nelson, M. W. 2005. A review of experimental and archival conflicts-of-interest research in auditing. In *Conflicts of Interest: Challenges and Solutions in Business, Law, Medicine, and Public Policy*, edited by D. A. Moore, D. M. Cain, G. Loewenstein, and M. H. Bazerman. Cambridge, U.K.: Cambridge University Press.
- Rigdon, E. E., R. E. Schumacker, and W. Wothke. 1998. A comparative review of interaction and nonlinear modeling. In *Interaction and Nonlinear Effects in Structural Equation Modeling*, edited by R. E. Schumacker, and G. A. Marcoulides, 1–16. Mahwah, NJ: Erlbaum Associates.
- Scholes, M., M. Wolfson, M. Erickson, E. Maydew, and T. Shevlin. 2008. *Taxes and Business Strategy: A Planning Approach*. 4th edition. Upper Saddle River, NJ: Pearson Prentice Hall.
- Schultz, E., and T. Francis. 2002. Companies profit on workers' deaths through "dead peasants" insurance. *Wall Street Journal* (April 19): 1.
- Securities and Exchange Commission (SEC). 2002. *Certification of Disclosure in Companies' Quarterly and Annual Reports*. Release Nos. 33-8124, 34-46427. Washington, D.C.: SEC.
- U.S. House of Representatives. 2002. The Sarbanes-Oxley Act of 2002. Public Law 107-204 [H. R. 3763]. Washington, D.C.: Government Printing Office.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be inserted using the "footnote" or "endnote" feature of the word processing software which will automatically number the footnotes throughout the manuscript with superscript Arabic numerals.

SUBMISSION OF MANUSCRIPTS

Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state upon submission that the work is not submitted or published elsewhere.
2. For manuscripts reporting on field surveys or experiments: If the additional documentation (e.g., questionnaire, case, interview schedule) is sent as a separate file, then all information that might identify the author(s) must be deleted from the instrument.
3. New manuscripts must be submitted through the Manuscript Submission and Peer Review System, located at <http://accr.allentrack.net>. The site contains detailed instructions regarding the preparation of files for submission. Revisions of manuscripts originally submitted before June 1, 2011 should be sent via email to the senior editor, John Harry Evans III, at tar@katz.pitt.edu. Please submit separate files for (1) the manuscript's title page with identifying information (not forwarded to reviewers), (2) the manuscript with title page and all other identifying information removed, and (3) any necessary supplemental files, such as experimental instructions and/or response memoranda on invited revisions.
4. A nonrefundable submission fee is required unless explicitly waived by the editor for invited revisions of previous submissions. The submission fee in U.S. funds is \$200.00 for members and \$400.00 for nonmembers of the AAA payable by credit card (VISA or MasterCard only). The payment form is available online at: <https://aaahq.org/AAAforms/journals/tarsubmit.cfm>. If you are unable to pay by credit card or have any questions, please contact the AAA Member Services Team at (941) 921-7747 or info@aaahq.org.
5. Revisions must be submitted within 12 months from the decision letter inviting a revision.

COMMENTS

The Accounting Review welcomes submissions of comments on previous *TAR* articles. Comments on articles previously published in *The Accounting Review* will generally be reviewed by two reviewers, usually including an author of the original article (to assist the editor in evaluating whether the submitted comment represents the prior article accurately) and an additional reviewer who is independent of the original article. If a comment is accepted for publication, the original author will generally be invited to reply. All other editorial requirements, as enumerated above, apply to proposed comments.

POLICY ON REPRODUCTION

An objective of *The Accounting Review* is to promote the wide dissemination of the results of systematic scholarly inquiries into the broad field of accounting.

Permission is hereby granted to reproduce any of the contents of the *Review* for use in courses of instruction, as long as the source and American Accounting Association copyright are indicated in any such reproductions.

Written application must be made to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, for permission to reproduce any of the contents of the *Review* for use in other than courses of instruction—e.g., inclusion in books of readings or in any other publications intended for general distribution. In consideration for the grant of permission by the *Review* in such instances, the applicant must notify the author(s) in writing of the intended use to be made of each reproduction.

Except where otherwise noted in articles, the copyright interest has been transferred to the American Accounting Association. Where the author(s) has (have) not transferred the copyright to the Association, applicants must seek permission to reproduce (for all purposes) directly from the author(s).

POLICY ON DATA AVAILABILITY

The AAA's Executive Committee policy (originally adopted in 1989, and amended in 2009) is that the objective of the Association-wide journals (*The Accounting Review*, *Accounting Horizons*, *Issues in Accounting Education*) is to provide the widest possible dissemination of knowledge based on systematic scholarly inquiries into accounting as a field of professional research, and educational activity. To fulfill this objective, authors are encouraged to make their data available for use by others in extending or replicating results reported in their articles.

MARCH 2014 PLACEMENT ADS

The deadline for free position ads to be included in this section of *The Accounting Review* is two months prior to the desired publication in the January, March, May, July, September, or November issues. Position ads, which are free with the purchase of a job posting in the AAA Career Center, should provide all relevant information about the available positions and must include contact information or application instructions for interested candidates. For more information on how to purchase a job posting in the Career Center, or for up-to-date, detailed information about the placement listings in this issue, please go to the AAA website at <http://aaahq.org> and click on "Career Center," or call our office at 941-921-7747.

UNIVERSITY OF WISCONSIN-MILWAUKEE, Sheldon B. Lubar School of Business invites applications for an Associate Dean Master's Programs position. Candidates seeking more information and application information should visit the UWM website at: <http://jobs.uwm.edu/postings/16370>. The University of Wisconsin-Milwaukee is an AA/EEO, committed to increasing diversity in recruitment and advancing our University as an inclusive campus.

RUTGERS, THE STATE UNIVERSITY OF NEW JERSEY, seeks applicants for full-time, non-tenure-track instructor positions within Rutgers Business School-Newark and New Brunswick to teach large classes (ranging from 100 to 450 students) for courses in Financial Accounting and Managerial Accounting. Applicants for these positions must have demonstrated excellence in teaching large classes and must have qualifications to meet AACSB (the business school accrediting body) standards for either academic or professional qualifications. Ph.D. desirable, ABD acceptable, minimum Master's Degree in area of teaching responsibility or Master's Degree combined with significant managerial experience. Apply electronically by sending your application via email to Professor Miklos Vasarhelyi, Search Committee Chair at: miklosv@andromeda.rutgers.edu. Only electronic submissions will be accepted. Applications will be accepted until the positions are filled.

UNIVERSITY OF SOUTH CAROLINA, School of Accounting is accepting applications for tenure-track positions to begin Fall 2014 with particular needs in the areas of tax, financial, audit, and systems. Candidates should possess a doctorate or expect to complete their doctorate by August 2014. New Ph.D. candidates should demonstrate the potential for teaching excellence and publishing in top-tier journals. Candidates for an advanced rank should have a record of teaching excellence and publishing in top-tier journals. Salaries are competitive and commensurate with experience and achievements. USC boasts an active research faculty with experienced and junior researchers in all areas. Submit *curriculum vitae*, working papers, and defended dissertation proposal to: Professor Mark Cecchini, Email: ACCTrecruit@moore.sc.edu. The University of South Carolina is an AA/EEO. Minorities and women are strongly encouraged to apply.

MISSOURI STATE UNIVERSITY, SPRINGFIELD, School of Accountancy invites applications for an August 2014 opening for an Assistant Professor or Associate Professor of accounting. All areas of specialization will be considered. The School of Accountancy has particular needs for teaching and research interests in one or more of the following areas: financial accounting, accounting theory, governmental/not-for-profit, systems, and auditing. Online teaching experience and professional certification are desirable. Candidates must have a sustained record of excellence in teaching, research, and service along with experience appropriate to the rank for appointment at the Associate Professor level. Candidates must be able to demonstrate job-related experience with and/or commitment to diversity in the work/academic environment. A criminal background check, at University expense, will be required for the successful candidate. For additional information contact Dr. Dick Williams, Director of the School of Accountancy, Phone: (417) 836-5414; Email: accountancy@missouristate.edu. Applications are accepted only online through the following sites: <http://jobs.missouristate.edu/postings/11100> (Associate Professor) or <http://jobs.missouristate.edu/postings/11102> (Assistant Professor). Consideration of applicants will begin December 9, 2013 and will continue until the position is filled. Missouri State University is an Affirmative Action/Equal Opportunity Employer.

UNIVERSITY OF HOUSTON-CLEAR LAKE (UHCL), School of Business is seeking one faculty member at the rank of Assistant Professor of Accounting, beginning August 2014. A doctorate in Accounting or an ABD nearing completion from an AACSB-accredited school is required. Preference will be given to candidates who are conducting discipline-based research and willing to teach cost/managerial accounting. The candidate must be eligible to work in the United States. Applications are accepted online at: <https://jobs.uhcl.edu>. To apply, please complete the application and attach a letter of interest and a *vita*. Please send three letters of recommendation and a transcript from your doctoral institution to: Michael Lacina, Accounting Search Committee, University of Houston-Clear Lake, Box 212, 2700 Bay Area Boulevard, Houston, TX 77058-1098. Review of applicants begins December 15, 2013. The University reserves the right to extend the search or not to fill the position. UHCL is an Affirmative Action/Equal Opportunity employer.

MIDWESTERN STATE UNIVERSITY, Dillard College of Business Administration invites applications for an accounting faculty position effective Fall 2014. Compensation is competitive. This position is designated as security sensitive and requires the finalist to complete a criminal background check. Doctorate degree with a concentration in accounting preferred; Master's Degree required. Preferred: Appropriate professional certification and related experience, record of teaching excellence in auditing and a continuing record of scholarly research and publication. Appointment will be at a rank appropriate to qualifications. The Dillard College is AACSB-accredited. Interested candidates should send a letter of application, current *curriculum vitae*, unofficial transcripts, and a list of three references to: Melissa Wright, Dillard College of Business Administration, Midwestern State University, 3410 Taft, Wichita Falls, TX 76308. Website: <http://business.mwsu.edu/accounting/index.asp>, Email: melissa.wright@mwsu.edu. EEO/ADAAA compliance employer.

WESTERN KENTUCKY UNIVERSITY invites applications for a tenure-track position in all ranks in the Department of Accounting. Appointment available August 2014. Earned doctorate with emphasis in Accounting (ABDs near completion and graduates from a Post-Doctoral Bridge to Business Program accounting track may be considered). Professional certification is desirable. Specialization in financial, auditing, or systems is desirable. Please submit the following: letter of application, *vita*, transcripts, student evaluations, and three letters of reference to: Accounting Search Committee, Western Kentucky University, 1906 College Heights Boulevard #11061, Bowling Green, KY 42101-1061. Electronic submission of these documents is acceptable at: Acct@wku.edu. The review of applications will begin on January 22, 2014. The position will remain open until filled. More information about this position can be obtained by contacting (270) 745-3895, or <http://www.wku.edu/employment/999822g.pdf>. Western is an Affirmative Action/Equal Employment Opportunity Employer.

WESTERN WASHINGTON UNIVERSITY invites applications for Dean of the College of Business and Economics. The Dean, a member of the university's executive leadership team, is responsible for academic leadership and administrative management for all programs within the college and for its outreach efforts. Accredited by AACSB International, CBE provides high-quality programs in business and economics, focusing primarily on undergraduate instruction in Accounting, Decision Sciences, Economics, Finance and Marketing, and Management. CBE faculty and staff support 1,300 undergraduate and graduate students. WWU, a comprehensive state university, enrolls 15,000 undergraduates, Post-Baccalaureate, and Graduate students, employs 600 FTE faculty, and consists of seven colleges and the graduate school. The campus overlooks Bellingham Bay and the San Juan Islands. Bellingham is nationally recognized for quality of life, outdoor adventure, and environmental stewardship. Visit: <http://www.wvu.edu>. Complete position description including responsibilities, qualifications, and electronic application instructions: <https://jobs.wvu.edu/JobPosting.aspx?JPID=4997>. Application screening begins January 13, 2014. EO/AA.

CALIFORNIA POLYTECHNIC STATE UNIVERSITY, SAN LUIS OBISPO, Orfalea College of Business, Accounting Area, invites applications for full-time, academic year, Assistant/Associate/Full Professor tenure-track positions in accounting taxation beginning Fall 2014. Rank and salary is commensurate with qualifications and experience. To apply, visit <http://www.calpolyjobs.org>, complete a required online faculty application, and submit application to Requisition #103073. Please attach a letter of interest, current *curriculum vitae*, and student evaluation summary. Please be prepared to provide three professional references with names and email addresses when completing application. REVIEW BEGIN DATE: January 23, 2014.

TENNESSEE TECH UNIVERSITY, College of Business invites applications for the Chair of the Accounting Department. a full-time, nine-month position, eligible for tenure upon appointment, subject to TBR approval. Must have Doctorate in Accounting or Business Administration with a major in Accounting, from an AACSB-accredited school. Scholarly and teaching achievements commensurate with the rank of Associate or Full Professor; demonstrated administrative capabilities; ability to accommodate and coordinate the activities and views of faculty; and ability to interact effectively with faculty, students, staff, administrators, and external parties are also required. Professional certification (CPA, CMA, CIA), experience with AACSB-I accreditation standards, and relevant professional work experience in accounting are preferred. The chairperson is expected to assume an active leadership role in curriculum matters, scholarly activity, maintenance of accreditation, strategic planning, and external relations. Salary is competitive and commensurate with education and experience. Interested applicants must apply at: <http://www.tntech.edu/jobs>. Applicants will be required to electronically upload a cover letter, *curriculum vitae*, copies of transcripts (official transcripts for all degrees conferred required upon hire), and contact information for five references who will be contacted at a later date. Submission of materials is the applicant's responsibility. Applications without all required materials are incomplete and will not be considered. Position open until filled. AA/EEO.

LOYOLA MARYMOUNT UNIVERSITY, Department of Accounting invites applicants for tenure-track positions at the Assistant or Associate level, effective Fall 2014. Candidates must have earned a Doctorate in Accounting by August 2014. All candidates should demonstrate a strong commitment to scholarly research and excellence in teaching. Candidates with teaching and professional experience in any area are encouraged to apply, with a preference for specialization in auditing, cost accounting, financial accounting (especially advanced accounting), or taxation. Salary is competitive. Loyola Marymount, a comprehensive university in the mainstream of American Catholic higher education, seeks professionally outstanding applicants who value its mission and share its commitment to academic excellence, the education of the whole person, and the building of a just society. Contact: Professor Lawrence Kalbers, Chair of Faculty Search Committee, (310) 338-2758; Email: Lawrence.Kalbers@lmu.edu. To apply for this position, please send an email with an attached letter of interest and current *curriculum vitae*. Please be prepared to submit three letters of recommendation and student evaluation summaries directly to the chair of the search committee upon request. LMU is an Equal Opportunity Institution actively working to promote an intercultural learning community. Women and minorities are encouraged to apply. For more information visit: <http://www.lmu.edu>

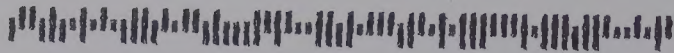
BENTLEY UNIVERSITY invites applications for a tenure-track position in accounting beginning in Fall 2014. We are seeking an individual with a specialization in auditing/systems or managerial, but will consider all qualified applicants regardless of area of specialty. All ranks will be considered. A qualified candidate must have a Doctoral Degree in Accounting (or expect completion of that degree by September 1, 2014), and a strong commitment to excellence in research and teaching. Bentley University and its accounting programs are AACSB-accredited. The University also holds EQUIS accreditation. The Accountancy Department offers undergraduate degrees, graduate master degrees, and a Ph.D. program. A recent accounting research rankings produced by BYU ranks Bentley as #1 in AIS, #1 in total Educational research, #6 in Auditing, and overall #30 in research productivity. Salary is competitive and support is available for summer research, summer teaching, course development, and for travel to professional meetings. Chair of the search committee is Jay Thibodeau. Interested candidates should submit all materials online to our Human Resources website at <http://jobs.bentley.edu> and include an application, current *vita*, letter of interest, working papers (upload using the "Other" link), a dissertation proposal, if available (Ph. D. candidates only; please upload using the "Bentley Additional Documents" link), evidence of excellence in teaching, and three references with names, addresses, and telephone numbers. As an Equal Opportunity Employer, Bentley is committed to building strength through diversity and welcomes applications from members of under-represented groups.

THE ACCOUNTING REVIEW
ISSN 0001-4826

PERIODICALS POSTAGE PAID
SARASOTA, FLORIDA
AND ADDITIONAL MAILING OFFICES

POSTMASTER
Send Address Changes To:

AMERICAN ACCOUNTING ASSOCIATION
5717 BESSIE DRIVE
SARASOTA, FL 34233



*****ALL FOR ADC 481

P-2 P38
14918 REVIEW 1
MARYGROVE COLLEGE
ORDER #A8554347
LIBRARY SERVICES
8425 W MCNICHOLS RD
DETROIT MI 48221-2546

639